

# THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

VOL. LXVII.

SATURDAY, SEPTEMBER 28, 1895.

No. 13.

## ORIGINAL ARTICLES.

### ON SOME CONDITIONS AFFECTING THE BEHAVIOR OF THE TYPHOID-BACIL- LUS IN WATER.

#### I.

BY EDWIN O. JORDAN, PH.D.,

ASSISTANT PROFESSOR IN THE UNIVERSITY OF CHICAGO.

It is now generally admitted that drinking-water is sometimes, perhaps usually, the vehicle of infection in typhoid fever. The exact conditions that befriended or antagonized the specific germ in water are therefore a matter of considerable importance, and have been made the object of study by many investigators. These studies, however, have led in many cases to results so conflicting, and have been based in general on methods and material so diverse, that the chief outcome has been confusion. A valuable summary of many of the most important experimental studies upon the vitality of the typhoid-bacillus in water is given by P. F. Frankland in the first report to the Water Research Committee of the Royal Society.<sup>1</sup> The existence of this tabulated statement obviates the necessity of any extended account of the literature, and I shall here refer only to those papers bearing more or less directly upon the points I wish to consider.

It is essential at the outset to emphasize the necessity of a thoroughgoing examination of the specific germ used in all experiments of this kind. I have elsewhere indicated<sup>2</sup> the special difficulties that environ the identification of the typhoid-fever bacillus, difficulties that make it imperative to record the source, age, and character of the culture used. Additional reasons for rigorous precaution in this particular will appear presently.

The culture of the typhoid-bacillus of which I have made use was obtained on November 26, 1893, from the fresh spleen of an individual who had died of a typical attack of typhoid fever. The bacillus was obtained in pure culture from different parts of the spleen. Parallel studies of this bacillus and of a culture of the *B. coli communis* freshly isolated from normal feces, together with several "varieties" of the colon-bacillus, gave assurance that the typical typhoid-organism had been isolated. Microscopic

examination showed a short, stout bacillus, with rounded ends and exhibiting active movement. It did not stain by Gram's method. Flagella were found distributed over the body of the cell, but the considerable variation in their number and size made it impossible for me to detect any fundamental difference from the flagella borne by the colon-bacillus. On gelatin plates the characteristic iridescent, irregularly notched expansion was produced; upon potato a moist, almost invisible pellicle was formed; milk was rendered slightly acid, but was not coagulated even after prolonged growth; in ordinary peptone-broth an abundant multiplication occurred, but there was no formation of indol; in peptone-broth to which formalin had been added (1:7000)<sup>3</sup> growth was inhibited. *No gas was produced in glucose-agar or in glucose-peptone-bouillon.*<sup>4</sup> On these grounds I was able to consider the organism I had isolated as the true typhoid-bacillus.

**Methods.** Before proceeding to compare the behavior of this organism with that of the colon-bacillus I must describe somewhat in detail my method of inoculation. It is necessary to dwell upon the manner in which the bacilli are introduced into the water, as there has been the greatest lack of uniformity in this respect, and as the importation of organic matter from the culture-media along with the bacilli has undoubtedly proved a fruitful source of error. Neglect of proper methods in this particular seriously vitiates, for example, the work of Meade Bolton,<sup>5</sup> De Giava,<sup>6</sup> Heraeus,<sup>7</sup> Straus and Dubarry,<sup>8</sup> Wolffhügel and Riedel,<sup>9</sup> and others.

The method of inoculation in my own experiments has been as follows: The typhoid-bacillus was always transferred (unless otherwise stated) from a 24-to-48-hour-old agar tube-culture grown at 37.5° C. Two or three needle-loops were re-

<sup>1</sup> Schild: Centralblatt für Bakteriologie, 1893, xiv, p. 717.

<sup>2</sup> The gas-test applied by Frankland ("The Behavior of the Typhoid-Bacillus and of the Bacillus Coli Communis in Potable Water," Proc. Roy. Soc., vol. 56), does not appear to me wholly satisfactory, owing to the fact pointed out by Smith ("The Fermentation-Tube," Wilder Quarter-Century Book, 1893) that all samples of beef-broth do not contain glucose. The use of ordinary melted gelatin-peptone, which Frankland recommends, could not always be relied upon for distinguishing between the typhoid-bacillus and the *B. coli communis*.

<sup>3</sup> Zeitschrift für Hygiene, 1886, i.

<sup>4</sup> Id., 1889, vi.

<sup>5</sup> Id., 1886, i.

<sup>6</sup> Archives de Méd. Expt., 1889, p. 1.

<sup>7</sup> Arbeiten aus d. kaiserlichen Gesundheitsamte, 1886, i.

<sup>1</sup> Proc. Roy. Soc., July 4, 1892. This summary, with additions and emendations, appears also in Micro-organisms in Water, by P. F. and G. C. Frankland, London, 1894.

<sup>2</sup> Journal of the American Medical Association, Dec. 22, 1894.

moved from the slanting surface of the agar-culture, great care being taken to carry as little as possible of the nutrient substratum along with the bacilli. A young culture is, in this respect, to be preferred to one that has grown for a longer time, as fewer dead bacilli are transferred along with the living ones. These loopfuls of bacilli were introduced into 50 c.cm. of sterile distilled water in a small Erlenmeyer flask, and the flask was shaken for several minutes in order to bring about thorough separation of the individual germs. One cubic centimeter of this inoculation-water was then added to a liter-flask containing the water in which it was desired to test the vitality. In order to determine how far this procedure introduces a quantity of nutrient substance sufficient to affect the longevity of the bacilli the following experiment was carried out: Three flasks, each containing exactly 1000 c.cm. of water drawn from the laboratory-tap, were sterilized in the steam-bath in the usual manner. To Flask A was added 1 c.cm. of inoculation-water prepared in the manner described and containing 3900 typhoid-bacilli. To Flask B were added 2 c.cm. of the same inoculation-water. To Flask C was added 1 c.cm. of inoculation-water; this flask was then sterilized by heat, cooled in running water as quickly as possible, and inoculated with 1 c.cm. precisely as Flask A.

	Flask A. (1 c.cm.)	Flask B. (2 c.cm.)	Flask C. (1 c.cm.) Sterilized inoculation- water + 1 c.cm.
Immediately after inoculation.	9000	18,000	9000
After 1 day . . . .	8415	17,325	9405
" 4 days . . . .	7390	12,870	8910
" 7 " . . . .	5775	9,936	6930
" 11 " . . . .	3905	7,260	3465
" 17 " . . . .	3025	7,205	2530
" 26 " . . . .	1485	2,805	1705
" 33 " . . . .	935	1,210	825
" 40 " . . . .	825	930	1045
" 48 " . . . .	200	84	142
" 62 " . . . .	81	9	2
" 83 " . . . .	5	5	0
" 103 " . . . .	0	0	0

A similar experiment gave a confirmatory result:

	Flask A'. (1 c.cm.)	Flask B'. (2 c.cm.)	Flask C'. (1 c.cm.) Sterilized inoculation- water + 1 c.cm.
Immediately after inoculation	3900	8000	3900
After 4 days . . . .	1870	3190	1815
" 20 " . . . .	58	113	42
" 41 " . . . .	6	4	9
" 55 " . . . .	2	0	3
" 75 " . . . .	0	0	0

These experiments show that the amount of food-material introduced by the method of inoculation that I have employed is not large enough to affect perceptibly the longevity of the bacilli.

These experiments also indicate that within certain limits the number of bacilli introduced

into the water plays no great part in increasing the total longevity. Frankland<sup>1</sup> suggests that among a large number of bacteria taken from a given source there may be some individuals capable of resisting a particular adverse influence, whereas with a smaller number there might not be any such individuals. While this is conceivably possible, my own results do not lend unqualified support to the view. I have found, for example, that after from 50 to 60 days, in such an experiment as I have described (*cf.*, *e. g.*, Flasks A and B after 40 days), the numbers are substantially the same when the initial number was three times as great in one case as in the other (6000 and 18,000). If it were attempted to increase the disparity of numbers to a greater extent than this, I think some risk would be run of introducing sufficient nutrient medium to complicate the experiment. There is at hand, moreover, a plausible explanation for the *apparent* influence of number in certain experiments recorded by other investigators. The number of living bacilli taken up by the loop from a 48-hour culture of freshly isolated typhoid-bacilli is for some reason larger, often very much larger, than the number obtained in a similar way from a 48-hour culture of typhoid-bacilli that has been under artificial cultivation for some months. There is here, therefore, a concurrence of two factors, namely, large numbers and short exposure to the influence of artificial cultivation. I believe, for reasons that I shall bring forward presently, that this last factor is of great importance. Large numbers of bacilli, then, are usually introduced from the very typhoid-culture that possesses great vitality, whereas when long cultivation has rendered the bacilli less able to survive in water smaller numbers are likely to be introduced. This fact, I think, is sufficient to explain the apparent connection between numbers and longevity that some experimenters believe to exist. In those cases in which very large numbers are dealt with (*e. g.*, over 100,000 per c.cm.) the influence of food-substance obscures the whole course of events, and inevitably leads to discordant results.

That the length of time the bacilli from a given culture are able to live in water is dependent on the length of time during which the culture has been under cultivation we have the result of the following experiment as evidence: A few days after I had isolated the typhoid-culture already mentioned, the bacilli were introduced into sterilized tap-water (Lake Michigan); in this instance living bacilli were found to be present after 93 days. From that time onward for thirteen months this particular typhoid-stock was under observation, and bacilli from it were introduced into sterile Lake Michigan

<sup>1</sup> Proc. Royal Society, vol. 56, p. 172.

water at frequent intervals. Care was taken by frequent transfers (agar-tubes) to maintain the vitality of the stock, as far as might be, unimpaired. For two or three months no very noticeable change was observed; but after that time the vitality of the bacilli in water began to wane. Through the whole series of thirteen months a fairly steady decline went on. In the last trial I have made the bacilli lived but 12 days. Intermediate periods were 83 days, 55 days, 41 days, 22 days, etc., the diminished vitality in each case corresponding with the time that had elapsed since the first isolation of the typhoid-stock. We cannot, therefore, if this culture be regarded as typical, escape the conclusion that *the age of the stock-culture employed in experiments upon longevity of the bacilli in water is a factor of prime importance.*

The water that was used in the experiments already described, as well as in those to follow, is drawn from Lake Michigan through the Sixty-eighth Street Tunnel, which supplies the southern part of the city of Chicago. The average composition for the summer and early autumn of 1893 was as follows:<sup>1</sup>

Parts per 100,000.			Average.
Total solids . . .	14.6	to 18.6	15.7
Loss on ignition . . .	3.0	" 3.7	3.4
Chlorin . . .	0.21	" 0.31	0.26
Oxygen consumed . . .	0.07	" 0.10	0.08
N as ammonia (albuminoid) . . .	0.0090	" 0.0127	0.0112
N as ammonia (free) . . .	0.0011	" 0.0039	0.0021
N as nitrites . . .	0.0001	" 0.0009	0.0003
N as nitrates . . .	0.0030	" 0.0070	0.0048

The composition of the water is subject to this not inconsiderable fluctuation, because the sewage that enters the lake is liable to vary in quality and quantity, and is more or less unevenly distributed by the currents and winds. I have not been able, however, to discover what influence, if any, these daily and weekly variations in the composition of the water have upon the life of bacteria introduced. The multiplicity of factors involved renders such a task almost hopeless.

All experiments have been conducted in the dark; the flasks have been kept at the ordinary room-temperature; and, in order to insure uniformity of conditions in each experiment, the flasks have been always placed close together in the dark closet. Through the winter months it has been found possible to keep the temperature fairly constant, from 18° to 21° C., but in summer the temperature of the dark closet has risen on some occasions to 25° or 26° C., and I have reason to suspect that this range of seven or eight degrees is sufficient to lead to disparate results. I have, consequently, included in this paper only such experiments as were carried out under similar temperature-conditions, deferring a discussion of special

temperature-influence to a second paper. In all cases, except when very high numbers were present (see Flasks D and E), one cubic centimeter of the infected water was used for plating.

*Steam-sterilized lake-water.* Proceeding now to a consideration of the results obtained with the typhoid-culture already described, we may first take up the case of steam-sterilized surface-water. Waters of this character have hitherto been the subject of the greater part of the experiments undertaken, but whether because of inaccurate and non-comparable methods, or because of diversity in the composition of the waters employed by different investigators, the results show a singular lack of concurrence. The longest duration of vitality observed by Hochstetter<sup>2</sup> in sterilized Berlin tap-water was seven days, and Mattei and Stagnitta<sup>3</sup> report a longevity of only four days, while, on the other hand, Straus and Dubarry<sup>4</sup> found that the typhoid-bacillus lived eighty-one days in the sterilized water of the polluted Ourcq, and Frankland<sup>5</sup> records a duration of life of "upward of seventy-six" days in steam-sterilized Thames water. Between these widely separated extremes many other periods of longevity in water have been recorded, but it is the extremes that concern us most.

The greatest longevity shown by the typhoid-culture with which I have worked was manifested in one of the early experiments soon after isolation, and extended over a period of ninety-three days.

Immediately after inoculation . . .	6435
After 28 hours . . .	4840
" 3 days . . .	5390
" 10 " . . .	4136
" 26 " . . .	3800
" 50 " . . .	1165
" 77 " . . .	49
" 90 " . . .	15
" 93 " . . .	16
" 110 " . . .	0

Succeeding trials showed, as I have already stated, a progressive decline of vitality. To take instances in which the initial numbers were low:

	4 months after isolation.	6 months after isolation.	13 months after isolation.
Initial number . . .	800	1540	1815
After 7 days . . .	660	935	1320
" 10 " . . .	660	364	78 (12 days)
" 20 " . . .	288	18	0

There is, moreover, considerable variation in the results of experiments made with exactly similar

<sup>1</sup> Arbeiten aus dem kaiserlichen Gesundheitsamte, 1887, ii.

<sup>2</sup> Annali dell' Istituto d'Igiene Sperimentale di Roma, 1889. I have not seen this paper, and only quote from Frankland's summary.

<sup>3</sup> Arch. de Méd. Expt., 1889.

<sup>4</sup> Proceedings Royal Society, 1894, vol. 56.

<sup>5</sup> Inspection of the tables of dates and longevity given by Frankland, on pp. 330 and 331 of his work, suggests a suspicion that the influence of the age of the culture may be traced in his experiments also.

<sup>1</sup> Allen Hazen: Engineering News, March 29, 1894.



numbers under similar conditions. (See, *e. g.*, p. 338, Flasks A and B.) This variation shows itself for the most part toward the end of the experiment in the more persistent vitality of a few individuals, and may cause so large a difference in the "period of longevity" as three weeks. A like variation, due without doubt to the variable hardness of individual bacilli, has been observed by other experimenters. This individual difference shows itself at its true value in the numerical tables, and it is noteworthy that the general course of the diminution of numbers is not altered, as it plainly is altered in the cases just tabulated, in which the age of the culture has modified the general vitality.

The behavior of the *Bacillus coli communis* in the steam-sterilized tap-water is in contrast to the behavior of the typhoid-bacillus. A culture of the *B. coli communis* freshly isolated from normal human feces, when introduced into a flask of water under the conditions just described, soon undergoes considerable multiplication. The following experiments, conducted at the same time, and parallel with the ninety-three-day experiment with the typhoid-bacillus already recorded, illustrate this point:

	D.	E.
Immediately after inoculation . . .	1,925 <i>B. coli</i> .	1,925 <i>B. coli</i> . 6,435 <i>B. typhi</i> .
After 28 hours . . .	2,035	6,160
" 3 days . . .	1,815	6,215
" 10 " . . .	77,000 <sup>1</sup>	299,750 <sup>1</sup>
" 16 " . . .	93,500 <sup>1</sup>	209,000 <sup>1</sup>
" 22 " . . .	66,000 <sup>1</sup>	200,750 <sup>1</sup>
" 26 " . . .	57,550 <sup>1</sup>	
" 50 " . . .	22,000 <sup>1</sup>	
" 90 " . . .	5,379	25,344 <sup>1</sup>
" 127 " . . .	7,260	
" 184 " . . .	148	660
" 262 " . . .	5	0

Therefore in ability not only to multiply but also to maintain vitality in water, *B. coli communis* differs from the typhoid-bacillus. Frankland<sup>2</sup> has recorded an experiment with steam-sterilized Thames water, similar to D, and giving a similar result; but the initial number of colon-bacilli is much larger (69,000) than I employed, and there is a more sudden falling off (from 117,000 to 1500 between fifty-six and sixty-eight days) than is shown in my experiment. The record of his experiment covers only seventy-five days. The result with Flask E is particularly interesting in this connection, as it shows a somewhat larger increase in numbers than Flask D. It is probable that this is due to the additional nutrient medium conveyed into the flask along with the 6435 typhoid-bacilli. This infinitesimal amount of organic matter is, as has been shown,

not sufficient to affect the typhoid-bacillus, but experiment E appears to indicate that the *B. coli* is more sensitive than the *B. typhi* to slight increments of food-material. The primary object of experiment E was to determine whether possibly symbiotic relations might obtain between the *B. coli* and the typhoid-bacillus, but the great multiplication of the former rendered this undertaking futile. On the first and third days a few colonies of typhoid-bacilli were identified on the plates (glucose-agar test), but on the tenth day and thereafter not a single typhoid-colony could be detected, although examinations of a large number of colonies were made repeatedly.<sup>1</sup> This, so far as it goes, indicates perhaps that the presence of the *B. coli* in water is hostile to the continued existence of the typhoid-bacillus, but the chance is obviously very great that typhoid-bacilli, if present (say 4136 among 299,000), might be entirely missed.<sup>3</sup> Guimbert states that the sowing of the *B. coli* and *B. typhi* together in water results in the disappearance of the latter (indol-test) after two or three days, but full details of the experiment are not given, and there are grave difficulties in the way of gaining satisfactory evidence on this point.

As a supplementary inquiry to these experiments with the colon-bacillus, a set of flasks, F, G, and H, were infected, two weeks later, with colon-bacilli derived from different sources, the other conditions being precisely similar to those in D and E.

The culture of the colon-bacillus with which F was infected was obtained from the spleen of an individual dying with typhoid fever. The spleen was procured for me through the kindness of Dr. A. P. Ohlmacher, who also sent me the following autopsy-notes:

"The patient was a male, aged twenty-four years. Death after six weeks from hemorrhage and sepsis due to perforation of ileum. Old adhesions around spleen, which was torn in removal on this account. Intestinal contents in abdomen and about the spleen."

From this spleen a typical colon-bacillus, agreeing in all respects with that isolated from normal feces, was obtained in pure culture and used for infecting Flask F.

The colon-bacillus used in infecting G was obtained from the fresh feces of the lion by Mr. J. M. Flint, and did not differ in any respect from the typical *B. coli communis* from the other sources.

Flask H was infected with a culture of the colon-bacillus, for which I am also indebted to Dr. A. P.

<sup>1</sup> In these cases 1 c.cm. of the infected water was diluted 1:50 with sterilized distilled water and 1 c.cm. of the mixture plated: the counts made from these plates were, of course, multiplied by 50.

<sup>2</sup> Loc. cit., p. 140.

<sup>3</sup> The Würtz-agar-method has not proved available in this connection because the large number of colonies of the *B. coli* on each plate have speedily reddened the whole medium and rendered the search for typhoid-colonies hopeless.

<sup>3</sup> La Semaine Médicale, 1894, No. 29, p. 230.



Ohlmacher. This was isolated from the pus in a case of appendicitis, and agreed in character with the cultures already described.

The results are as follows:

	F. Inoculated with <i>B. coli</i> from spleen.	G. Inoculated with <i>B. coli</i> from feces of lion.	H. Inoculated with <i>B. coli</i> from appendicitis pus.
Initial number	10,065	2,695	2,255
After 2 days	9,900	2,420	2,915
" 8 "	17,600	2,850	6,000
" 12 "	18,400	46,750	8,900
" 22 "	14,750	96,250	5,650
" 36 "	35,400	60,500	5,670
" 134 "	5,940	10,890	1,295
" 248 "	Not continued	4,675	Not continued

In two respects there is perfect agreement between the cultures of the *B. coli communis* from the four different sources, namely, they all exhibit multiplication and they all show long-continued vitality. The interesting difference in the amount of multiplication that occurs may be referable to racial variation, and suggests, in the cases of F and H, an approximation toward the typhoid-bacillus. I have not yet been able to make a trial of the behavior in water of the "varieties" of the typhoid-bacillus. It is likely that experiment in this direction would add to the number of varying "grades of vitality" in water. It is possible that there may be discovered a graded series of forms intermediate between the true typhoid-bacillus on the one side, which perishes in water under one-hundred days and never multiplies, and, on the other, the true colon-bacillus, which undergoes a very considerable multiplication in water and may maintain its vitality for upward of two-hundred-and-sixty-two days. The more tenacious vitality of the "typical" colon-bacillus, obtained from whatever source, together with its ability to thrive and multiply on very small quantities of nutrient material, is, however, a characteristic that separates it from the "typical" typhoid-bacillus and emphasizes the functional difference known to exist in other respects.

*Steam-sterilized distilled water.* Experiments upon the vitality of the typhoid-bacillus in sterilized distilled water have not led to more harmonious results than those with sterilized potable waters, although the more uniform quality of the water might have led us to anticipate greater uniformity of results. Hochstetter<sup>1</sup> records five days as the longest duration of vitality that he observed, while Straus and Dubarry<sup>2</sup> record a longevity of from thirty to thirty-five days, and Slater<sup>3</sup> has recently found living typhoid-bacilli in water fifty days after inoculation.<sup>4</sup>

<sup>1</sup> Arbeiten aus dem kaiserlichen Gesundheitsamte, 1847, ii.

<sup>2</sup> Archives de Méd. Expt., 1889, p. 1.

<sup>3</sup> Journal of Pathology and Bacteriology, June, 1893, p. 468.

<sup>4</sup> Frankland, in the tables already spoken of, refers to Braem as asserting that the typhoid-bacillus lives for 188 days in dis-

In the last-mentioned two cases, however, the importation of considerable quantities of organic matter renders the results nugatory. Straus and Dubarry, indeed, find practically no difference between longevity in potable water and in distilled water, a fact in itself sufficient to arouse suspicion of some obscuring cause. My own experiments leave no doubt of the fact that, when the same typhoid-culture is introduced under the same conditions into ordinary surface-water and into distilled water, the bacilli invariably perish more rapidly in the latter than in the former. The longest duration of vitality in distilled water that I have observed is eighteen days. In this work with distilled water I have used both the method of direct plating and that of adding sterile bouillon to the infected flask, without, however, any difference in outcome. Four months after isolation of the typhoid-culture the mortality in distilled water was very great:

Initial number	2090
After 1 day	197
" 3 days	1
" 6 "	0

Twelve months after isolation the difference between the behavior in tap-water and in distilled water was still manifest:

	Distilled water.	Tap-water.
Initial number	1815	1815
After 7 days	0	1320
" 12 "	0	78

The striking difference between the behavior of the typhoid-bacillus in ordinary potable water and in distilled water led me to search for the cause, as it is clearly important to attack the problem from this side, as well as by experiments under "natural conditions." That the typhoid-bacillus is able to retain its vitality and virulence for a long time in unsterilized water is a point amply established by the history of epidemics, as well as by laboratory-experiments *in vitro*. The latter have so far chiefly been confirmatory of the evidence from epidemiology. The specific conditions of water that favor or hinder infection have remained, however, very obscure.

One conspicuous difference between distilled water and ordinary surface-water lies in the absence of organic nitrogenous matter (organic nitrogen or albuminoid ammonia) from the former. With a view to discovering more precisely what effect organic matter in sterilized water has upon the

tilled water. This is certainly an error, for Braem explicitly states (Ziegler's Beiträge, 1890, vol. vii, p. 11) that the large numbers of bacilli, and the concomitant food-material, which he found it necessary to use in his experiments, must have influenced materially the longevity. His observations, moreover, were purely microscopic, not cultural, and he expressly disclaims any implication as to the viability of the "tief dunkeln, dicken Stäbchen," which he found "nach 196 Tagen in sehr reichlichen Menge."

typhoid-bacillus the following experiments were undertaken. I hope to supplement them in a short time with similar experiments upon unsterilized waters.

In order to secure uniform conditions, a standard peptone-solution was prepared by dissolving one gram of peptone in 500 c.cm. of redistilled water. In this solution analysis showed that there were present 12.6 parts per 100,000 of organic nitrogen (Kjeldahl method).<sup>1</sup> Accurately measured quantities of the peptone-solution were added by means of graduated pipets to a series of flasks each containing exactly 1000 c.cm. of sterilized distilled water. These were inoculated with typhoid-bacilli in the usual way. The organic nitrogen-content of each flask (parts per 100,000) is indicated by appended decimals:

	Z <sup>1</sup> (0.0126 org. N.)	Z <sup>2</sup> (0.0252 org. N.)	Z <sup>3</sup> (0.063 org. N.)	Z <sup>4</sup> (0.126 org. N.)	Z <sup>5</sup> (0.0 org. N.)
Initial number	2090	2090	2090	2090	2090
After 1 day	216	600	880	2585	197
" 3 days	1	51	175	2100	1
" 6 "	0	0	13	495	0
" 10 "	...	...	0	3	
" 17 "	...	...	...	4	

Other experiments gave in the main confirmatory results, as:

	Z <sup>6</sup> (0.0126)	Z <sup>7</sup> (0.0252)	Z <sup>8</sup> (0.063)	Z <sup>9</sup> (0.126)	Z <sup>10</sup> (0.252)	Z <sup>11</sup> (0.0)
Initial number	1925	1925	1925	1925	1925	1925
After 1 day	935	275	2475	4950	2035	990
" 3 days	87	26	2035	3300	2365	
" 18 "	1	0	2	15	468	2

The addition of one or two c.cm. of the standard peptone-solution (0.0126 and 0.0252 organic nitrogen) appears to be in general without effect upon the longevity of the bacilli, although the first experiment that I have recorded showed grades of vitality singularly adjusted to the rising increments of organic matter (*cf.* Z<sup>1</sup>-Z<sup>6</sup>). There is a slight multiplication of the typhoid-bacilli, when from 5 to 20 c.cm. of the peptone-solution (0.0630-0.252 organic nitrogen) are added, and some experiments even seemed to indicate that with 10 and 20 c.cm. this multiplication might give rise to very large numbers. These latter experiments, however, were complicated by the effects of summer temperature, and I shall pass them by for the present.

Without entering upon a catalog of the large number of experiments of this kind that I have made, it will suffice to state that the results are similar throughout: *the bacilli invariably live longest in those waters containing the largest amount of organic nitrogen.* It is a noteworthy fact that this holds true even when no appreciable multiplication occurs. (*Cf.*, e.g., Z<sup>1</sup> with Z<sup>3</sup>, and Z<sup>8</sup> with Z<sup>11</sup>.) This latter

interesting circumstance is open to the interpretation either that the lives of individual bacilli are conserved by the presence of the organic matter, which acts as a kind of elixir of life, or that the more hardy bacilli are able to multiply for one or two generations on the slight amount of peptone supplied them, a multiplication that, however, is masked by the mortality occurring among their less resistant companions. For obvious reasons the second explanation seems to me the more plausible.

These results, showing the influence of organic matter upon the longevity of the typhoid-bacilli in distilled water, suggested a similar series with ordinary surface-water. My earliest experiments in this direction demonstrated that a quantity of peptone-solution (1 c.cm. = 0.0126 organic nitrogen) insufficient to affect the longevity in distilled water brought about considerable multiplication in the tap-water, and led to a long-continued persistence of vitality.

	X <sup>1</sup> (0.0126 org. N.)	X <sup>2</sup> (0.063 org. N.)	X <sup>3</sup> (0.0 org. N.)
Initial number	1,540	1,540	1,540
After 1 day	1,500	1,925	1650
" 6 days	ca. 20,000	Countless	935
" 10 "	26,730	456,500	364
" 23 "	19,800	170,500	18
" 80 "	1	0	0

In following up this result an unexpectedly delicate reaction upon the addition of the peptone was observed. The introduction of 0.5 c.cm. and even of 0.1 c.cm. of the peptone-solution (0.0063-0.0012 organic nitrogen) brought about multiplication.

	X <sup>4</sup> (0.0126 org. N.)	X <sup>5</sup> (0.0063 org. N.)	X <sup>6</sup> (0.0012 org. N.)	X <sup>7</sup> (0.0000)
Initial number	605	605	605	605
After 3 days	6,160	3,190	500	358
" 6 "	ca. 20,000	?	15,000	342
" 9 "	ca. 30,000	15,000	...	288
" 94 "	29	0	0	0

The course of these experiments was broken into by the complication caused by summer temperature, and the inquiry was not resumed until the typhoid-culture was weakened by twelve months of artificial cultivation. The results at this time, however, fully substantiated the earlier conclusions, and, indeed, imparted to them additional emphasis. With this weakened culture no appreciable multiplication was detected, but the addition of 0.0012 part per 100,000 of organic nitrogen was still sufficient to affect the longevity of the bacilli.

	X <sup>8</sup> (0.0)	X <sup>9</sup> (0.0012)	X <sup>10</sup> (0.0025)
Initial number	1815	1815	1815
After 7 days	1320	1925	
" 12 "	78	...	990
" 15 "	0	165	385
" 64 "	...	0	2

These experiments demonstrate that the amount of organic nitrogen of a certain kind in a given

<sup>1</sup> This determination was kindly made for me by Mrs. R. H. Richards, of the Massachusetts Institute of Technology.

sterilized water plays an important rôle in lengthening the life of the typhoid-bacillus in that water. In this connection it is interesting to note that Frankland<sup>1</sup> found in his comparative experiments with sterilized Thames water, Loch Katrine water, and deep well-water that the longevity of the typhoid-bacillus is distinctly greatest in the Loch Katrine water, and that this contains the most organic matter. "Not improbably," he says, "these circumstances are connected together." The difference between distilled water and ordinary surface-water, however, shows that other factors, presumably those concerned with the mineral content of the water, may be involved.<sup>2</sup>

The upshot of the experiments recorded in this paper may be stated briefly as follows:

1. The age of the typhoid-stock influences greatly the life of the bacilli introduced into water, a freshly isolated stock possessing distinctly greater vitality than one that has been under cultivation for some months.

2. The typhoid-bacillus, when introduced with proper precautions into sterilized Lake Michigan water, does not multiply, but may, under certain conditions, maintain its vitality for upward of ninety-three days.

3. The colon-bacillus, on the contrary, under similar conditions undergoes rapid multiplication and may remain alive for upward of two-hundred-and-sixty-two days.

4. In redistilled water the typhoid-bacillus perishes much more speedily than in the water of the lake.

5. The quantity of organic matter (peptone) in redistilled water influences fundamentally the life of the typhoid-bacillus; so small an increment as 0.0126 organic nitrogen (parts per 100,000) causing a perceptible lengthening of life.

6. In sterilized lake-water the addition of a still smaller quantity of organic nitrogen—0.0012—affects the longevity of the typhoid-bacilli introduced.

#### A NEW METHOD OF BANDAGING THE THUMB.<sup>3</sup>

BY JULIUS SELVA, M.D.,  
OF BOSTON, MASS.

THE art of bandaging was born with the fathers of medicine, Hippocrates, Galen, Soranus, Glaucus, and other eminent physicians; and although in

ancient times bandages were "plainly intended more for parade than use,"<sup>4</sup> yet we must look at those rudimentary appliances as the foundations of our present system of bandaging. The "scapha," or boat-like bandage; the "gallia gladiatoris," or bandage like a gladiator's helmet; the "regium," or bandage like a crown, have all gone into disuse; but from these antique forms were developed, by the process of evolution, in the course of centuries, the more efficient and simpler bandages of modern times.

The essential rules of bandaging remain always the same: a roller is turned around a limb, trunk, or head, either in circular, spiral, reverse, recurrent, or figure-of-8 form.

It is not necessary to emphasize in this paper the great use and importance of bandages, as there is hardly any one operation in surgery practicable without their assistance.

I have devoted much time to the application of bandages during my experience as a surgical interne to the Boston City Hospital and the Children's Hospital, and about a year ago I devised a new method of bandaging the thumb. Before describing this method, an account will be given of the different thumb-bandages that have been used by various authors.

The thumb-bandages that are on record may be classified into four different types: (1) The spiral. (2) The reversed. (3) The spica or figure-of-8. (4) The combination of these forms.

The spiral bandage for the thumb is probably the oldest variety. It is described in the works of Heister,<sup>5</sup> and also in recent times by Gross<sup>6</sup> and others.

Thillaye,<sup>4</sup> Gerdy,<sup>5</sup> Liston,<sup>6</sup> Velpeau,<sup>7</sup> Jamain,<sup>8</sup> Guillermin,<sup>9</sup> Heath,<sup>10</sup> Leonard,<sup>11</sup> Smith,<sup>12</sup> Mears,<sup>13</sup> and others, described later on a bandage of the thumb, which is practically a simple spica of the thumb and wrist. It begins by two circular turns round the wrist. After the last turn, which should be terminated upon its radial side, the head of the roller is to be directed from the external to the internal side of the thumb, passing between this and the index-finger, to return and cross its base, and be carried again about the wrist. These double circulars are repeated to form a spica, till the whole bandage is exhausted. This bandage makes no provision for covering the distal phalanx of the thumb.

Wales<sup>14</sup> crossed bandage of the thumb is about the same as the preceding ones. It begins by circular turns at the wrist, and then a successive number of spica turns are made to cover in the thumb, forming an *ascending spica*. When the turns are made from the base downward the spica is said to be *descending*. The bandage terminates at the wrist. The end of the thumb is left uncovered.

Hopkins<sup>15</sup> bandage does not differ from that of Wales.

<sup>1</sup> Loc. cit., p. 251.

<sup>2</sup> The evidence adduced by Frankland (Proc. Roy. Soc., vol. lvi, pp. 225-227) to support the view that typhoid-bacilli can be educated for aquatic life is inadequate. The quantity of organic matter inevitably introduced in such experiments as he has described is, perhaps, sufficient to account for the phenomena that he is inclined to attribute to "a prolonged and gradual training in more and more aqueous culture-media."

<sup>3</sup> Demonstrated before the Boylston Medical Society, March 29, 1895.



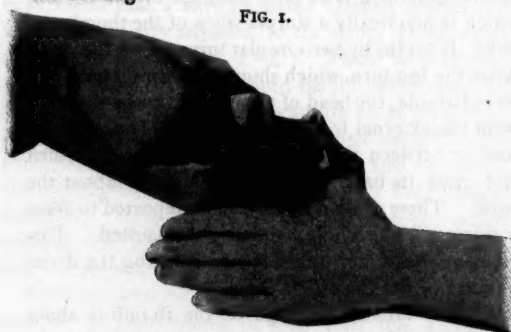
Pye's<sup>10</sup> spica of the thumb is begun with a few turns round the wrist from within outward if the outside of the thumb is to be most supported, and in the opposite direction if the ball is the part requiring the firmer pressure.

Hill,<sup>11</sup> Hunter,<sup>12</sup> Wyeth,<sup>13</sup> Wharton,<sup>14</sup> and others describe a bandage of a characteristic type. It commences round the wrist, but the first turn is carried at once beyond the last joint, turned once or twice round the last phalanges, then returned by *spiral* turns or spiral reverses to the metacarpo-phalangeal joint, where the ball of the thumb is covered by spica turns round the thumb and wrist, each figure-of-8 alternating with a *circular* turn round the wrist. Thus this bandage is a combination of the circular, spiral reverse, and figure-of-8 turns. The end of the distal phalanx is not covered.

In the *American Text-book of Surgery*<sup>21</sup> is described a similar spica of the thumb, which starts at the wrist. According to the figure, it ends at the wrist and does not cover the pulp of the thumb.

The *gauntlet* and *demi-gauntlet* bandages described in ancient and modern text-books are applied to the wrist and finger, and they are not especially adapted to the thumb. No mention is made of thumb-bandages in the classic works of Bell,<sup>22</sup> Castle,<sup>23</sup> Symes,<sup>24</sup> Cooper,<sup>25</sup> Smith (Stephen),<sup>26</sup> Esmarch,<sup>27</sup> Hamilton,<sup>28</sup> Bryant,<sup>29</sup> and others.

Inasmuch as all the bandages that have been used for the thumb are connected with the wrist, their efficiency is more or less impaired by the motions of this joint. Their application presents the following features: (1) They begin at the *wrist*. (2) They end at the *wrist*. (3) The distal phalanx of the thumb is not completely covered. (4) The hand—*i. e.*, the *metacarpus*—does not participate in the bandage.

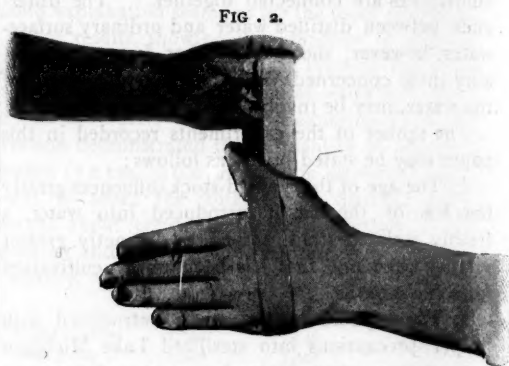


Beginning of bandage—application to abducted thumb.

The new method of bandaging the thumb that I have devised is as follows: The length of the bandage is from 4 to 6 meters and the width from  $2\frac{1}{2}$  to 3 centimeters, according to the size of the hand.

*Application.* The hand is held in a position of semipronation, the thumb pointing upward and

being *abducted* at an angle of about  $50^\circ$ , so that the end of the distal phalanx is in line with the heads of the second and fifth metacarpal bones. The first stage of the application of this bandage consists in completely covering the terminal phalanx. The initial end is applied to the outer lateral surface of the second phalanx of the thumb, near its base, and the roller is carried along this surface, over the palmar side of the pulp of the distal phalanx, to



First turn about hand. Figure-of-eight of thumb.

the inner lateral surface of the second phalanx; this turn is held in place by means of the index-finger and thumb of the left hand of the operator, as shown in Fig. 1; then the roller is returned in a *recurrent* form to the place of origin; this last turn should overlap the preceding one, and should be placed in as dorsal a position as possible; then the roller is directed downward and inward over the dorsum of the terminal phalanx and is turned around the very tip of the pulp (Fig. 1), forming a loop that will cross at a point corresponding to the center of the nail. Having thus covered the terminal phalanx, the roller is carried over the dorsum of the hand as low down as the head of the fifth metacarpal bone, the thumb all the time being held *abducted*; it is then turned over the palm of the metacarpus to the dorsal surface of the terminal phalanx, at the point of the first crossing, thus forming a figure-of-8 turn (Fig. 2). This turn can be repeated over itself if the first one is not firm enough for starting. Then a series of successive figure-of-8 turns are made, each overlapping its predecessor and being about half a centimeter apart, forming an *ascending spica*; these turns are carried as high as the carpo-metacarpal articulation, and the bandage is ended by pinning it at the palmar surface of the ball of the thumb, or by splitting and tying it around the metacarpus.

This bandage is essentially a figure-of-8 form, the *smaller* or *lower* loops of which go around the first and second phalanges and part of the ball, and the *larger* or *upper* loops pass and overlap each other around the metacarpus and upper portion of the

ball of the thumb. The bandage can be applied as high as necessary, and in order to completely cover the very highest portion of the ball the lower loops should be applied nearer together, or they can be repeated over themselves at the palmar surface of the base of the second phalanx, whereas the upper loops can be applied successively in an ascending form over the surface of the ball. Of course, if there is no indication, the *recurrent* portion of the bandage may be omitted, in which case the roller is started by one or two circular turns around the *end of the distal phalanx*, and then the rest of the bandage is applied as before. If, after starting, the roller should be directed first over the *palmar* surface of the metacarpus, in making the upper loop, the crossings of the figure-of-8 turns will be situated on the palmar side of the thumb. Fig. 3 shows the thumb-bandage applied.

FIG. 3.



The bandage completely applied.

It will be noticed that these figure-of-8 turns not only overlap themselves, but they also exert an *ascending compression*, which adds strength to the bandage and produces efficient immobilization of the thumb. As these turns are being applied, the thumb, which originally was in a position of *abduction*, is gradually approached to the metacarpus and it becomes more and more *adducted*.

This bandage, unlike the others that have been previously described, does not begin or end at the wrist; it is a bandage that is applied to the phalanges of the thumb and to the metacarpus, and consequently the motions at the wrist-joint do not interfere with the efficiency of the appliance. Provision is made by this bandage for completely covering the terminal phalanx of the thumb.

*Uses.* 1. To apply dressings to any portion of the thumb, or to the dorsal or palmar surface of the metacarpus.

2. For immobilizing fractures, dislocations, or sprains of the thumb, in which cases the application of strips of sticking-plaster, in the shape of the bandage described, is useful.

3. For the retention of splints.

4. To arrest hemorrhage.

[NOTE.—In bandaging the arm to the thorax, and the leg to the pelvis I have used a method similar to that described.]

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## TONGUE-TRACTION AS A CARDIAC STIMULANT.

BY H. A. MCCALLUM, M.D.,

OF LONDON, ONTARIO;

PROFESSOR OF PHYSIOLOGY AND LECTURER ON CLINICAL MEDICINE IN WESTERN UNIVERSITY.

IN June, 1892, Dr. J. A. Mullin, of Hamilton, in a discussion on chloroform before the Ontario Medical Association, said that he regarded forcibly pulling on the tongue several times as the most powerful excitator of respiration. I determined to make use of the suggestion at the first opportunity, and the manipulation was, therefore, resorted to in all anesthetic accidents with animals used in the physiologic demonstrations during the following winter sessions.

We soon learned to place considerable confidence in this novel measure. It was about one year thereafter that I first saw the recommendation of "rhythmic tongue-traction" in asphyxia by Laborde, of Paris.

Early last session, after some successful demonstrations of "blood-pressure" on a large dog, the sternum was rapidly cut away and the thoracic organs exposed. After stimulation of the vagus, to note the effect on the exposed mammalian heart, the dog was allowed to die by asphyxia. While my associate Dr. O. Weld (now of Vancouver, B.C.),

was chloroforming a rabbit, to show the effect on the blood-pressure of depressor-nerve stimulation, the class clamored for an exhibition of tongue-traction.

The dead dog was brought into service, the tongue was grasped with an artery-forceps and violent rhythmic traction was practised. The result was a surprise; instead of the diaphragm undergoing the expected rhythmic movements, the apparently dead heart, which was exposed, commenced to beat and underwent vigorous movements for some time. A number of dogs, cats, and rabbits were anesthetized and similarly experimented upon. Drs. Weld, Williams, and Neu were present at some of these experiments, and it was soon determined that tongue-traction is a powerful cardiac stimulant.

In the cat, tongue-traction stimulates diaphragmatic more readily than cardiac movements; in the dog and rabbit the effect is more marked on the heart.

Tongue-traction increases not only the rate of an exposed and failing heart but also the vigor of its contractions. It appeared at first that this method might stimulate the heart by lowering the blood-pressure, through the tissues of the neck acting as a pump on the carotids and aorta, or through inhibition of the vasoconstrictor center. To determine the truth or falsity of this theory some animals were allowed to die of hemorrhage and the effects of tongue-traction noted. The effects on these hearts were, if anything, more marked than in animals dying with cardiac distention.

On some animals both vagi were divided, but the results of tongue-traction on the heart were unaltered. We were therefore led to conclude that its mode of action was on the cardio-accelerating center in the medulla. This view was supported by the fact that, especially in the cat, the respiratory movements were also excited. Marey<sup>1</sup> has made some investigations to determine how tongue-traction stimulated the respiratory movements. He could abolish the results on the diaphragm by cutting either the sensory nerves of the tongue or the phrenics.

Laborde speaks in the most confident terms of the superiority of this method of restoring cases of asphyxia, and I am satisfied it is a method that will succeed, even when other methods fail, because it acts as both a respiratory and cardiac stimulant. It could and should be combined with some good method of artificial respiration in anesthetic accidents, cases of drowning, asphyxia and syncope from any cause.

On several occasions we were able to restore animals by the combined method when we otherwise failed. It is easy to exhaust this effect on the medulla, so that if tongue-traction be practised

on the animal before the heart ceases to beat, no amount of traction can restore the contractions when the heart-beat has ceased.

## CLINICAL MEMORANDA.

### NEPHRECTOMY FOR TUBERCULOSIS OF THE KIDNEY IN A PATIENT ALWAYS A RESIDENT OF COLORADO.

By WILLIAM S. BAGOT, M.D. (Dublin University),  
OF DENVER, COL.;

PROFESSOR OF GYNECOLOGY IN THE UNIVERSITY OF DENVER; GYNECOLOGIST TO ST. LUKE'S AND ST. JOSEPH'S HOSPITALS; FORMERLY SENIOR ASSISTANT PHYSICIAN TO THE ROTUNDA LYING-IN HOSPITAL, DUBLIN; FELLOW OF THE BRITISH GYNECOLOGICAL SOCIETY, ETC.

MRS. P., aged twenty-two years, who had been married six months and had had no miscarriages, was sent to me by Dr. James A. Hart, of Colorado Springs. It was learned that her paternal grandfather had died of pulmonary tuberculosis. Her father, a German by descent, began to complain of pulmonary trouble when in his twentieth year, and was sent to Colorado on account of pulmonary tuberculosis nine years afterward. He died at the age of forty-nine years. As far as can be ascertained no other immediate relative has suffered from tuberculosis.

Mrs. P. was born in Colorado. When three years old, she had a severe fall, attended with some injury to the right knee, after which the part remained swollen and inflamed. For seven years her parents consulted physicians without receiving any definite diagnosis or obtaining relief for the condition. At the end of this time they consulted Dr. Sandford, of Colorado Springs, who diagnosed the trouble as tuberculosis of the knee-joint, and performed the operation of erosion, with a successful result. From that time, her tenth year, the patient remained in apparently perfect health till about Christmas, 1893, when she had an attack of influenza, after which she suffered from some trouble with her bladder and kidneys. For the latter trouble she was treated by Dr. James Hart during February and March, 1894, but soon discontinued treatment, as she felt much relieved. She was married in the following June. In September she began to feel ill and noted frequent urination, with fever, and suffered from severe pain in the region of the right kidney, which radiated up toward the right shoulder and down into the thigh on the same side. She then sent for Dr. Hart, who examined her and found a large tumor in the right hypochondriac and lumbar regions.

On November 81, 1894, I saw the woman in consultation with Dr. Hart and corroborated his opinion that she was suffering from tuberculosis of the kidney. She was then extremely emaciated, being reduced from 160 to about 100 pounds in weight; the evening temperature was 101.5° F., the pulse 116 per minute. A large tumor occupied the right renal region, and a greatly thickened ureter could be felt crossing the brim of the pelvis. Through the vagina the ureter could be felt to be thicker than one's little finger. The urine was loaded with pus. On palpation the left ureter and kidney seemed normal. Examination of the lungs revealed slight consolidation of the right apex.

<sup>1</sup> La Tribune Médicale.



The patient was admitted to St. Joseph's Hospital on November 24, 1894, her temperature on that evening being 101.5° F. and her pulse 108. She was kept in bed, given a milk-diet and iron (Blaud's pills) in an endeavor to build up her strength, as she was extremely anemic and so weak that I did not think she could stand any operative interference. After six days had elapsed, during which her condition had become, if anything, worse, her evening temperature still reaching 101.6° F., and her pulse 112, I decided to operate. Accordingly, on December 1, 1894, I performed nephrectomy, using ether as the anesthetic.

The kidney was exposed by means of Czerny's incision; then, in order to gain more room, as the organ was very much enlarged, measuring 10 inches by 6 inches, a second incision was made somewhat obliquely from the center of the first downward and forward for about three inches, without opening the peritoneum. On exposing the kidney, the tuberculous process was found to have transgressed the tunica propria and to have invaded the perirenal fat. While enucleating the kidney, which was very adherent in places, it ruptured, and a large quantity of pus escaped. This was at once washed away by flushing the wound with sterilized salt-solution. Before securing the pedicle the ureter, which also was tuberculous and thicker than a little finger, was separated as far down into the pelvis as possible and resected. The renal vessels were then ligated with catgut and the kidney removed. A small rubber drainage-tube was inserted into the proximal end of the ureter and by a suture secured into the lower end of the Czerny incision. The wound was flushed out with sterilized salt-solution and all tuberculous foci in the perirenal fat scraped as thoroughly as possible with a sharp curet. The incisions were then closed with silkworm-gut, leaving sufficient space to pack the cavity with strips of plain sterilized gauze, interspaced with strips of iodoform-gauze (5 per cent.). After this a sterilized gauze dressing was applied.

For the first seventeen hours after the operation the temperature gradually rose from 92.2° F. till it reached 103° F.; *i. e.*, 1.4° higher than it had been the evening before operation. During the next twelve hours it dropped to 100.4° F., this being the highest point reached during the remainder of the convalescence. The temperature became normal on the tenth day. The wound was dressed and the packing renewed on the third day. After this the dressings were renewed every second day, the stitches being removed on the twelfth day. Recovery was somewhat retarded by the appearance from time to time of small tuberculous foci in the wound. These were scraped away with a sharp curet and dusted with flowers of sulphur, which seemed to act better than balsam of Peru or iodoform, both of which I had tried. The patient left the hospital February 3, 1895, a small sinus still existing. When last seen, in June, 1895, this sinus had almost closed; all symptoms had disappeared and weight had increased to 157 pounds.

This case is of interest from the fact that it is, as far as I can learn, the first case of nephrectomy for tuberculosis of the kidney contracted in Colorado.

*Dr. L. Galassi*, Professor of Special Pathology in the University of Rome, died on August 31st, of a cardiac affection.

# LOSS OF CONJUGATE DIVERGENCE, OR PARALYSIS OF EXTERNAL ROTATION OF THE EYES.

BY HOWARD F. HANSELL, M.D.,

OF PHILADELPHIA;  
CLINICAL PROFESSOR OF OPHTHALMOLOGY IN THE  
JEFFERSON MEDICAL COLLEGE, ETC.

IN a somewhat hasty search through accessible literature I have been unable to find a case analogous to that herewith described. Writers on mental and ocular disease refer briefly to conjugate deviation of the eyes and paralysis of lateral movement as associated conditions in hemiplegia when the lesion has been either determined, or is supposed, to be in the pons, corpus striatum, or optic thalamus, or the cortex of the brain. The case to be reported differs essentially from other cases of conjugate deviation in that, while there was an utter inability to turn either eye outward a single degree beyond the median line, there was no deformity or abnormal turning of either eye—that is, there was no paralysis and no conjugate deviation. Schieff and Adamuk claim that the corpora quadrigemina regulate in part the eye-movements: "Of the posterior pair the right one governs the movement to the right, the left one that to the left. Irritation of both bodies of the same side causes deviation of the eyes to the opposite side of the anterior bodies in the median line upward, and of the posterior downward and inward." Graefe assumes the existence of a separate center for the associated movements to explain "co-ordination-spasm," or co-ordination-paralysis, or a different action of each eye—thus, one up and the other down, a "disjunction of co-ordination."

Dr. F. X. Dercum, under whose care the patient was, and to whom I am indebted for the opportunity to make the ocular examination, has kindly furnished me with his notes.

*Cerebellar Disease presenting Unique Ocular Symptoms.* A. P., an unmarried young woman, aged seventeen years, presented a negative family history. The patient is the youngest of three children, and was healthy at birth. At the age of six months she was severely ill with scarlet fever and diphtheria, making a "poor recovery." She had a persistent discharge from both ears and numerous abscesses in the glands of the neck. She was under medical attendance almost constantly until she was seven years of age, but improved somewhat at this time. Two years ago she began to menstruate, and since that time has noticed difficulty in walking, first referred to the right leg. When she came under observation the girl presented excessive ataxia in all her movements, and swayed greatly on standing. The muscles generally were weak, but no localized paresis could be detected. The knee-jerks were increased on both sides. There was no tendency to spastic rigidity, and the legs were perfectly flaccid. There was no anesthesia anywhere. The pupils were large and responsive to light. Marked deafness existed, a watch being heard only on contact.

Examination of the ears by Dr. S. MacCuen Smith revealed a perforation of both drums. It was found that the patient was unable to turn the eyes to the right or the left. She could readily fix all objects in the median line; convergence was maintained, and was normal in range. As soon as the object was moved to one or the other side and the girl was requested to look at it,

she utterly failed to direct the axis of either eye toward it. Many attempts were made, so that no misunderstanding could arise, but all were fruitless. There was no diplopia and no squint, either manifest or latent. Conjugate deviation was absent. The eyes were turned neither to the right nor to the left, but steadily maintained a position of parallelism or convergence. None of the muscles was paralyzed; the pupils were equal in size and normal in action; refraction presented a low hyperopia; vision was equal, 20/70; accommodation, 10 D.; exophoria for distance, 2°; abduction, 10°; adduction, 20°; there was no limitation of the fields for white or colors, and no evidence of inflammation; the media were clear and each fundus was normal. The discs showed no peculiarity or sign of previous or present neuritis or papillitis.

Extended observation will be necessary to determine the pathology. The diagnosis lies between hysteria and intracranial disease. If the former is present, no explanation so well fits the case as that offered by Allen Starr<sup>1</sup> of the presence of a lesion, microscopic it is true, in the nerve-cells or fibers, resulting probably from impaired nutrition from an altered condition of the blood or of the bloodvessels, and secondary changes in the excretions. If intracranial disease exists, the location of the lesion is extremely uncertain. The absence of paralysis and the loss of co-ordination point to the cerebellum or to the cerebral cortex as the site of the lesion.

Risien Russell<sup>2</sup> concludes that probably all ocular movements are represented in the cerebral cortex. The ocular effect produced by ablation of part of the cerebellum is paralytic, but whether from the direct influence of the cerebellum on the muscle or indirectly through the loss of the cerebellar influence over the cerebrum is uncertain.

#### EXCISION OF THE COCCYX FOR CONSTANT PAIN RESULTING FROM AN UNUNITED FRACTURE.<sup>3</sup>

BY LEWIS H. ADLER, JR., M.D.,

OF PHILADELPHIA;

PROFESSOR OF DISEASES OF THE RECTUM IN THE PHILADELPHIA POLYCLINIC, ETC.

IN presenting the history of the following case, I by no means do so with the idea of advocating so radical a procedure as an incision in all, or even in the majority, of patients suffering with pain in the region of the coccyx. In the present instance, in which all other means had been tried and proved futile, and in which pain was induced by every movement of the bone, I think that the operation was fully indicated.

The particulars of the case are as follows:

Mrs. A. W., aged forty-three years, was brought to my office February 21, 1895, by Dr. I. Grier Barber, of Danville, Pa. The family history was excellent. The patient had always enjoyed the best of health until six years ago. In December of that year (1889) she slipped and fell on the edge of a board-walk, striking the lower portion of the spine. She was unconscious for a short while. Ecchymosis developed slowly over the region of the sacrum. Extreme pain was experienced and a sen-

sation of something moving when she attempted to stir, which was especially marked in making an effort to sit. The bowels would not move without an enema, and at such times the pain was so severe as to produce faintness. About four months after the accident the woman was hardly able to walk a distance of three or four squares. During all this time she had consulted no physician.

A year-and-a-half after the reception of the injury she was examined under chloroform-anesthesia, and the coccyx was found to be united to the sacrum at a right-angle. The bone was refractured, but the operation was unsuccessful in diminishing the woman's suffering. About this time she lost flesh and had a purulent discharge from the bowel every week or so, the quantity averaging from one to two tablespoonfuls.

For about a year previously to consulting Dr. Barber the patient had been under the care of a physician who had been treating the bowel-trouble with local measures, but without any appreciable relief. Upon consulting Dr. Barber (a week before I saw her), he had Dr. Hoffa and Dr. P. C. Newbaker, both of Danville, examine the patient. As a result of their investigation they wrote me that the rectum was very painful and sensitive, principally at the lower end of the coccyx. A segment of the coccyx about an inch long was quite movable, as though there was an ununited fracture. It was thought possible that the bone might be necrosed and that an internal fistula might exist, but these suppositions could not be confirmed.

Upon making a digital examination of the rectum I found about an inch of the coccyx very freely movable, and the structures overlying the lower portion of the sacrum were adherent to the bone.

On February 22d the patient was etherized, the sphincters were stretched, and a curvilinear incision was made over the sacrum and around one side of the coccyx. The tissues were dissected so as to expose fully the seat of trouble. The upper portion of the fractured end of the coccyx was found to be necrosed, and the entire bone with the exception of the tip was removed.<sup>1</sup> The structures surrounding the bone were denser than usual and were firmly adherent to the coccyx, so much so, in fact, that considerable care was necessary in order to avoid wounding the rectal wall.

A rubber drainage-tube was introduced into the wound and the incision was closed with silk-worm-gut sutures. Iodoform was dusted over the part and the usual antiseptic dressing applied. It was necessary to catheterize the patient for some ten days after the operation. On the third day the bowels were moved by the administration of fractional doses of calomel and sodium bicarbonate, followed by a saline. On the fourth day the wound was found to be infected, presumably from contact with fecal matter. The stitches were all removed, as well as the drainage-tube. The wound was washed with a mercuric-chlorid solution (1:2000), followed by a 2 per cent. solution of creolin. The cavity was packed with iodoform-gauze.

On March 4th a sinus full of pus was discovered at

<sup>1</sup> Dr. H. Augustus Wilson, of this city, first proposed to me the idea of allowing the tip of the coccyx to remain if possible, when excising the body of that bone, for the reason that the muscular and ligamentous attachments thereto maintain their usefulness much better than when the entire bone is removed.

<sup>2</sup> The Western Reserve Medical Journal, May, 1895.

<sup>3</sup> Journal of Physiology, 1894 vol. xii, i.

<sup>4</sup> Read before the Philadelphia County Medical Society.

the lower angle of the wound. This was opened freely under local anesthesia induced by ethyl chlorid. From this time on the patient's recovery was uninterrupted though somewhat slow, and she was discharged, practically free from pain, on April 17th. Since then the patient has written to say that she has gained in weight about sixteen pounds.

1650 ARCH STREET.

#### HEMATEMESIS IN A CHILD TWO YEARS OLD.

BY HENRY M. FISHER, M.D.,  
OF PHILADELPHIA.

ABOUT midnight of August 23, 1895, J. P. began to vomit blood, about "a cupful," at first bright red. The vomiting recurred at intervals of half an hour for two or three hours. At 9 o'clock on the following morning a considerable amount of entirely undigested green corn was ejected, and with it blood-clots and some black liquid blood. When I saw the child, two hours later, he was not markedly prostrated, with a pulse of 120. There was no nasal obstruction, and there were no signs of recent epistaxis. The blood actually vomited had not been saved, but the sheet upon which the child lay was deeply stained, and there were also numerous clots upon it. I ordered a milk-diet, with lime-water, and one-half dram of magnesium sulphate every hour till a free movement of the bowels occurred, and directed the mother to take the child to a hospital in case of recurrence of the hematemesis.

Five days later the parents reported that the magnesium sulphate had caused several black movements, and that there had been no further bloody vomiting.

It seems that the child had partaken freely of green corn and cantaloupe on the evening before the bloody vomiting took place.

#### BACTERIOLOGIC NOTE.

##### THE USE OF THE AUTOCLAVE FOR STERILIZING NUTRIENT GELATIN.

BY WYATT JOHNSON, M.D., AND J. E. LABERGE, M.D.,  
OF MONTREAL, CAN.

[From the Laboratory of the Board of Health of the Province of Quebec.]

AN impression seems to exist in many bacteriologic laboratories that the sterilization of nutrient gelatin by means of the autoclave is impracticable, owing to the tendency of this medium to lose its power of setting firmly when exposed to a temperature above the boiling-point of water. Our experience has shown the contrary to be the case, and the results obtained by a single sterilization in the autoclave have been fully as satisfactory as those obtained by fractional sterilization at 100° C. on successive days, so that we now employ the autoclave by preference to avoid delay and uncertainty. The steam-pressure employed is 3/4 of one atmosphere (equal to 115° C.), saturated steam for fifteen minutes, after the gelatin has been filtered and filled into tubes.

In a series of comparative tests in which half of the nutrient medium was prepared by fractional sterilization in an Arnold sterilizer, that prepared in the autoclave was equally transparent, retained the desired degree of alkalinity (usually 2 per cent. acid to phenolphthalein),

and remained firm in plates or roll-tubes at a temperature of from 24° to 25° C. In no case have we had any spontaneous growth in the tubes after this sterilization.

We claim no priority for these observations, as the autoclave at 105° C. seems to have been employed for some years in several French laboratories for sterilizing gelatin,<sup>1</sup> though this fact does not appear to have become as generally known as it might be. In the laboratory of the Board of Health of the Province of Quebec attention was first attracted by the fact that some gelatin, sterilized in the autoclave through a mistake, retained its power of setting. In our experience a temperature of 105° C. has not always been sufficient to insure perfect sterilization.

With reference to the temperature we employ, it may be well to mention that the test-tubes and small bottles which we use are made of very thick glass, so that the heat penetrates slowly. In working with thinner ones a lower pressure or a shorter time may be found to give the best result. We have employed gelatin giving a firm jelly, as considerable difference exists between the different brands in respect to their melting-points.

#### MEDICAL PROGRESS.

*Tubercle of the Optic Thalamus with Intention-tremor and Tetanoid Rigidity.*—EDWARDS (*Lancet*, No. 3753, p. 260) has reported the case of a boy, six years old, who seven weeks before coming under observation was noticed to drag his right leg in walking. Two or three days later it was observed that in taking food the child held his spoon in the left hand, and upon his mother insisting upon the use of the right hand for that purpose this latter began to tremble. The trembling was at first slight, but gradually grew more marked. For three weeks there had been severe frontal headache and for four days vomiting. For a short time vision seemed to be impaired. The tremor in the right arm was a coarse and jerky movement, disappearing entirely when the arm was at rest. The movements came largely from the shoulder. There was no nystagmus or syllabic speech. The right arm and leg were distinctly weaker than the left. There was no facial paralysis. There was well-marked ankle-clonus on the right side, but none on the left. The wrist-jerks, as well as the epigastric, abdominal, cremasteric, and plantar reflexes were alike on both sides. There was marked rigidity on passive movement of the right knee-joint. The patient had difficulty in standing unsupported, but not increased by closing the eyes; nor was there any sign of reeling in one or other direction. With assistance he was able to walk and the gait was distinctly ataxic. Sensation was universally normal. The functions of the bladder and rectum were normal. The vomiting was controlled upon the administration of bismuth, but the headache persisted. While under observation well-marked optic neuritis developed in the left eye. There appeared to be also inflammation of the right nerve, but the observation was interfered with by a conjunctivitis, with slight clouding of the cornea, and concomitant blepharospasm. The other cranial nerves

<sup>1</sup> See Thoinot: *Précis de Microbie*, p. 83.



appeared to be normal. Toward the close of the ophthalmoscopic examination, which was somewhat prolonged, the patient was suddenly seized with an attack of tetanoid rigidity. He became unconscious, with the head bent back, the spine arched, and the whole body extremely rigid, the teeth clenched, and the eyes turned to the right. The face preserved its natural color. This condition persisted for about three minutes, when the muscles gradually relaxed and there was a return to complete consciousness. No clonic contractions followed. It was now learned that the child had had three similar attacks the night before. Large doses of potassium iodid were administered, in conjunction with mercurial inunction. At a later date three more attacks of tetanoid rigidity occurred, the patient falling asleep, worn out, after each. On awaking he moaned constantly and complained of his head. On another occasion five attacks occurred in rapid succession, and upon the following day death ensued. At the post-mortem examination, held sixteen hours after death, rigidity was very marked. The skull-cap was normal; the membranes were congested, but did not show any signs of inflammation. A distinct, firm tumor could be felt on the left side of the brain. On opening the left lateral ventricle in the usual way a very considerable quantity of clear, serous fluid gushed out. There was no excess of fluid in the right lateral ventricle. The tumor was about the size of a large walnut and was found to be situated in the left optic thalamus. A vertico-transverse section through the tumor and the left cerebral hemisphere showed that the growth was pressing on the left internal capsule. The floor of the fourth ventricle presented no signs of pressure, and sections through the medulla and the upper cervical region of the cord appeared normal. Microscopic examination of the growth showed it to be undoubtedly tuberculous.

**Acute Rheumatism in the Sequence of Tattooing.**—WILLIAMS (*British Medical Journal*, No. 1800, p. 1440) has reported the case of a militiaman, seventeen years old, who, three days after an extensive tattooing of the left forearm, complained of pain, swelling, and tenderness of the left wrist. A day later acute left-sided pneumonia developed, but rapidly subsided. The left shoulder, knee, and ankle were successively involved in the inflammation, and a cardiac bruit developed. Finally, chorea appeared as a complication, limited for a time to the left side, but shortly spreading to the right, where rheumatic inflammation was attacking the joints. The last, however, quickly subsided, leaving a general though mild chorea and a permanently damaged heart.

## THERAPEUTIC NOTES.

**The Treatment of Malignant Neoplasms with the Toxin of Erysipelas.**—CZERNY (*Münchener medicinische Wochenschrift*, 1895, No. 36, p. 833) cites two cases of carcinoma in which a favorable influence was exerted by an intercurrent attack of erysipelas, and four cases of sarcoma in which a similar result was brought about by injections of the toxin of erysipelas. The following conclusions are offered: Injections of sterilized, but not filtered, mixed cultures of the streptococcus erysipelatosus and the bacillus prodigiosus induce rapid elevation of tem-

perature, often with rigor, gastric disturbance, fulness in the head, and delirium; sometimes with labial herpes, and almost always without local inflammatory manifestations. The intensity of the reaction varies with the individual, the amount of toxin injected, and as to whether the fluid enters the interstitial tissues or the bloodvessels. These manifestations subside in the course of a few hours without permanent disturbance of the general condition. Frequently repeated injections are followed by loss of appetite, emaciation, anemia, and general apathy. The injections are capable of exerting a specific influence upon the sarcomatous growths, and under favorable circumstances may lead to recovery. The neoplasms become saturated with serum and then disintegrate and are absorbed, or soften, with necrosis and exfoliation. As the result in the individual case is uncertain, this mode of treatment cannot replace the operative method or render this superfluous. It is, therefore, for the present indicated only in cases of inoperable or recurrent neoplasms. It is possible that the method may be useful after operations for sarcoma, to prevent recurrence. In cases of carcinoma the injections appear at most to retard the growth of the tumor, but not to lead to recovery.

**Apolysin** is the name given by NENCKI and JAWORSKI (*Allg. med. Centr.-Ztg.*, No. 60-62; *Centr. für die ges. Ther.*, Sept. 1895, p. 528) to a new antipyretic and analgesic belonging in the same chemic group as phenacetin. The substance appears as a whitish-yellow, crystalline powder, of slightly acid taste, of feeble odor, difficult of solution in cold but readily soluble in hot water, and melting at 72° C. It is also soluble in alcohol and in glycerin. The drug was employed in a wide range of cases, in doses of from 7½ to 22½ grains, up to 45 grains daily. In febrile cases it acts as an antithermic, the decline of temperature being attended with the disappearance of other symptoms, especially pain. In cases of neuralgia it acted as an anodyne, diminishing the hyperesthesia, shortening the duration of the individual attack, and often causing disappearance of the symptoms. The action of the drug was rapid and certain and was unattended with unpleasant effects. Its use is contraindicated when the stomach is empty and when secretory hyperacidity is present.

**The Treatment of Whooping-cough.**—ULLMANN (*Jahrb. f. Kinderheilk.*, B. 40, H. 1; *Centr. f. ges. Ther.*, Sept. 1895, p. 560) undertook to determine the influence of the treatment in the open air of children suffering with whooping-cough, as compared with treatment indoors. In one case the interval between the attacks of coughing averaged one-and-a-half hours in the open air and forty-eight minutes indoors. In another case the child coughed one-and-a-half times and in a third two-and-three-fourths times oftener indoors than out. In a further case observed for nineteen days it was found that the cough was twice as frequent during the day indoors as out-of-doors, and nearly as frequent during the day out-of-doors as during the night in well-ventilated apartments. Other good effects of the treatment out-of-doors were observed in the less intensity of the attacks, the improvement of appetite, the infrequency of vomiting, and the betterment in the general condition.

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SATURDAY, SEPTEMBER 28, 1895.

## CONCERNING SPECIALISM.

WE have lately been treated in numerous articles to criticisms of specialism in medicine, varying from placid censure of its most obtrusive extremes to violent denunciation of the very fact itself and of all its important illustrators. At first we were inclined to sing in chorus. The recent hypertrophy of specialism, the outrageous extremes to which certain exponents have driven it, especially in America, with our orifacialists, graduated tenotomists, female castrationists, ossicle-excisionists, and all the rest, have always loomed big before our eyes. Assent would therefore be an easy task, especially when the impertinent exaggeration of morbid extremists seems to delight in thrusting itself upon the attention, and insulting all sense of self-control with examples of manifest evil. It may, however, very properly be asked if there is no good to be extracted even out of things most woful. All evil has its uses—at least for purposes of warning.

The first and most obvious answer to the critics—we mean the critics *sans phrase*, the *delendo est Carthago* fellows—of specialism is that the contempt poured upon the greatest extremists does not apply to the vast majority of specialists. It is unjust to

single out the maniacal hobby-riders and sneer *ex uno disce omnes*. All are not alike. The hobby-riders are few and exceptional; the preponderant majority do not deserve the strictures and are not affected by them. It is sheer nonsense to say that the great body of specialists know nothing of general diseases, that "a man who looks only into the ear cannot see far into the nose," that "they who view female life through the vagina will have little respect for the stomach," that the surgeon will cut rather than cure, and that for stricture of the rectum the oculist will apply glasses or snip eye-tendons. The people who say such things are cantankerous, and try to say sharp things instead of true things. Argument by epigram may be amusing, but, like government by epigram, it ends nowhere or worse than nowhere. The makers of *mots* never say the truth, only the error they impale. Their desire is not to instruct or to be honest, but to make people say, "How smart!" But if they wished to be truthful instead of supercilious, careful instead of captious, they would take special examples or at least types of extremism and pour upon them the vials or larger vessels of justifiable wrath, and not rain it upon the just and the unjust alike, thereby in themselves logically and literally illustrating the very indiscrimination of which they so bitterly complain in others.

The *tu quoque* argument is usually a weak one, but in the present contention it is exceptionally strong and convincing. There is not a specialist in the land but sees every day instances of mistaken diagnosis (and hence of treatment) upon the part of the general physician. What oculist, from examination of the eyes alone, has not pointed out to the general physician the existence of hitherto unsuspected nephritis or circulatory disease? The aurist is constantly emphasizing the facts of ear-disease as a cause or medium of communication of septic cerebral disease, or as a result of respiratory abnormalism. The daily rehearsal of tragedies in the specialist's office, plainly due to errors in diagnosis of the general physician, is pitiable. "My doctor told me my headaches were due to congestion and anemia, and never to let any oculist meddle with my eyes;" "he didn't tell me to have my teeth looked after;" "he didn't examine my urine, although I've had pain in the back for a year;" "he didn't examine my sputum, although I've had a hacking cough for a long time," etc., etc. We do not mean, nor do we think, that the general phy-

sician makes more errors than the specialist; we mean that the specialist is not the only medical sinner. It is our conviction that the average specialist is as much alive to the symptoms and importance of systemic disease as is the average general physician, and certainly no one would deny that he is also as much alive to the abnormalisms of special organs *other than those of his specialty*. We sharply emphasize the word *average* as applied to both classes. A recent diatribe against specialism tirelessly reiterates that the present-day variety is empiric. The charge is, of course, too true of all medicine, but to say that is particularly true of specialism rather than of general practice is topsyturveyism and squarely opposed to a proper reading of the facts. What is the very stronghold of empiricism, what the enemy that the most extreme specialism is heroically fighting to carry by storm—what but general therapeutics?

But if, as vehemently averred, the specialist is the superficial empiricist and faddist, why does the general physician not annihilate him? Why, instead of annihilation, does he encourage the ever-increasing custom of sending the specialist his patients to treat for their special diseases? The specialist's fate lies at the disposal of the family physician. Without reference-cases the specialist would soon languish and die his supposably deserved death. A homely saw says, "The proof of the pudding is in the eating." Is it not plain that the family physician is learning that the specialist has his uses, that he has special ability and excellence gained by special application and experience, and that in our day of the infinite division of function and profundity of research he must often be called in to supplement the general therapist? Has the family physician not learned by bitter experience that never again can one mind encompass more than a small fraction of the knowable, and that his duty to his patient is henceforth to learn where and of what nature is the root of the evil, and, beyond a certain range of ailments, he must advise consultation with the specialist?

There still linger about the fringes of our scientific world some wonderful specimens of an antediluvian age, strange stranded relics of megalosaurean vanity, or of slimy avarice—the quacks—we mean those within the profession—who "treat all diseases," they who "turn no patient out of their own office." Heaven help them—or, rather, help their patients! In the meantime, heaven not

helping, the "empiric" specialist must patch up the bungler's work as best he may after the "all-round man" has finished with his ignorance and mistakes. It is simply maudlin nonsense to contend that any one mind can longer compass or master all branches of medical science. It is painfully evident that any one specialty demands the most devoted application of the very best mental ability and training in order to keep abreast of the giant-progress of the time. The inevitable and ruthless march of scientific progress has divided and will continue to divide the practice of medicine into departments or specialties. There are undoubtedly unfortunate aspects and results of this subdivision of work; they are most glaring as well in other sciences as in medical science, but the law, *divide et impera*, is as predestined and inobviable as gravitation, and to rail at it is utter fatuousness. It is wiser to guide it rightly and utilize it shrewdly.

All critics say the specialist should enter upon special work only after thorough training in general medicine, and that is true; but it goes without saying, and in its final analysis it means that the schools must give a far more thorough grounding in general medical essentials than they do. If it means that every person must practise general medicine for ten years before taking up a specialty—then there is something to say on both sides of the question. We are unable to see how the delivery of a thousand women or the treatment of five hundred cases of typhoid fever can much help one to be a better aurist, or bacteriologist, or ophthalmologist. It is easy to demand much of the young physician, ambitious, short-lived (*ars longa, vita brevis*), and eager to be at his real life-work. He certainly should be consulted a little about it all. The advice savors a little of the old maxim about learning to swim without going near the water, or in another aspect it looks like advising an *experimentum in corpore vili*; but mistakes in specialty-practice are hardly more expensive than those in general practice. We are not advising the young graduate to plunge into a specialty at once; we are contending for the due weighing of circumstances, ability, training, and all that.

And what does one mean by specialism? Where will one draw the line? Should every family physician be also a surgeon? Assuredly not! It is simply impossible. It is to-day truer than ever that no man can serve two masters, much less a dozen



or two. Should every physician rely upon his own judgment as to refraction? Should he treat obscure aural diseases? Should he be his own bacteriologist? Not in any case if he have a particle of modesty, or honesty, or interest in his patient's health.

Beyond all question and despite all abuses, the rise of specialism has been the condition of medical progress. How many thousands, nay millions, of people are there to-day blessed with ocular health and ability to carry on the duties of civilization by reason of the work of Graefe, Helmholtz, Donders, and their followers. Would any of the discoveries in ophthalmology and their applications have been made without the specialist? What has revolutionized surgery but specialism? What is now revolutionizing all medicine but the work of the specialist in bacteriology? Has there been a single great discovery in modern medicine that is not the work of the specialist, or of men who, if living, would to-day be specialists? Is specialism not the absolute *sine qua non* of promised discovery in the future?

Finally, mark it well, the family physician is now quite as much a specialist as anybody else, and this whole pother of discussion is a mere meaningless war of words and misunderstandings. The general physician does not treat more diseases, perhaps even less, than the physician who confines himself to a single organ. In the progress or process of subdivision the generalist has become the veriest specialist. Moreover, almost every disease has or may have its effects upon every special organ, and no specialist who ignores general diseases, and diseases of other organs, will henceforth be able to hold "the pace" set for him by his broader-minded and more comprehensive rivals. Thus the evils of specialism—evils that we acknowledge and deplore—are in a fair way of curing themselves. The way as advised by high authorities should not be backward, but on through! Specialists will not, cannot renounce their peculiar work, and become general physicians; science and humanity cry out against such an absurdity; and the way through is the right way. The specialist is already becoming the general physician in the sense that he knows the limits of his own knowledge, and does not try to do what he cannot do, but advises consultation with those who can and who do know. This also the specialist family-physician is fast learning, and so he is broadening also into general practice, and no longer puts "drugs of which he knows little into a

body of which he knows less, for a disease of which he knows nothing."

Theoretically the essence of the controversy and of the whole matter consists in the attainment of the vantage-ground of accurate knowledge of at least one organ, and of the diseases of that organ. From this standpoint one cannot worse but better survey the whole field of medicine.

But the practical lesson of it all is that every one—*i. e.*, every "specialist"—shall learn to confine himself to the work he is competent to do, and beyond that to advise consultation with others more competent to treat certain organs or diseases; and the advice to consult the specialist family-physician has been, is, and will remain that very frequently given by his co-worker in other specialties.

## EDITORIAL COMMENTS.

*The Culture-test in Diphtheria and the Question of Quarantine.*—The examination of the throats of persons convalescent from diphtheria has revealed the presence of the Klebs-Loeffler bacillus even as late as three months after all clinical manifestations of the disease had ceased. In the majority of cases studied the bacilli have been found virulent up to the fourth week of the disease. Not only in persons who have just recovered from an attack of diphtheria, but also in the throats of nurses and caretakers virulent bacilli have been discovered in the absence of the least symptom of disease.

Hitherto it has been the custom in Philadelphia among officials who have to do with contagious diseases to fumigate the premises of persons suffering with diphtheria as soon as the attending physician pronounced the patient able to leave his room, and the physician usually allowed this when the membrane had disappeared from the throat and the individual seemed in good health. The placard was removed forty-eight hours after fumigation, and though children were kept out of school for two weeks longer, they were allowed to mingle freely with their comrades and relations.

Since the inauguration of bacteriologic laboratories and the use of culture-tests, fumigation has been delayed in such cases in which cultures have been made, until the bacilli are no longer found; the placard is left for the same period of time, and the patient is confined to the house and isolated from the other children.

In a large, roomy house, with garden-space, prolonged confinement of a healthy child can hardly be considered a hardship, either for the individual or his family; but for the poorer classes, who occupy seldom more than three rooms, and whose families are usually large, isolation for a period of from four to six weeks is not only difficult of execution, but is also a very serious matter for the little patient. Yet just here the quarantine is most needed, because the street is the playground of the poorer children, the contact with playmates is close, and children are numerous. It is difficult to make the parents understand that the danger is not yet over when

the child seems to be in a perfectly healthy condition, and they will be less rigid in maintaining a quarantine.

Unfortunately but a small number of the cases of diphtheria that occur in a city are submitted to the culture-test; and as the Board of Health of Philadelphia at least accepts the attending physician's statement that the house is ready for fumigation, the children of one family may be allowed to leave the house two weeks earlier than the children of another family, living, perhaps, in adjoining houses, simply because no cultures were made in the one case and were made in the other.

The neighborhood will soon discover that the first doctor dismisses his cases much sooner than the second, and will, of course, sound his praises above the other. The children of the former will be able to attend school two weeks earlier than those of the latter, the new rule or Act of Assembly simply stating that a period of thirty days shall elapse "after the discharge by recovery or death of the person last afflicted" and "the thorough disinfection of the premises," and as disinfection of the premises does not occur in culture-cases until the bacilli have disappeared, the thirty days date from the end of such time.

The intent of the law evidently was to exclude all cases until the danger of infection was past; if by bacterial examination the danger is proved to be over, then surely the purpose of the law is fulfilled and the child should be allowed to enter upon his school-duties.

The proper thing to do, it seems to us, would be to require the culture-test in all cases, and to refuse to raise the quarantine until the bacilli are no longer found present.

This would put the matter on a scientific basis, and would not allow those physicians who make use of the test to be placed at a disadvantage for doing so. If this were carried out, the accommodations at the Municipal Hospital would probably have to be increased, as more children would be sent there, owing to the impossibility of effective isolation in private houses for so long a time, whereas at the hospital the convalescent children have the use of the grounds and porches, and therefore do not suffer because of their confinement.

In the therapeutics of diphtheria much attention will in future have to be paid to expelling the bacilli in as short a period of time as possible. In the July and August numbers of the *American Journal of the Medical Sciences* Dr. F. H. Williams reports the results of investigations in this direction, and claims to be able to cause the bacilli to disappear from the throat on or before the nineteenth day of the disease. The antitoxin-treatment has not as yet been shown to have any direct influence in shortening the time of residence of the bacilli, although the membrane usually disappears earlier with its use. The bacilli persist after the membrane has left, and they may be present without the formation of membrane.

In justice, therefore, to the community, to the patient, and to the doctor, we repeat that the Health-Inspector should require a culture-test in every case before raising the quarantine; otherwise those who make use of this test will be unfairly discriminated against. Apartments occupied by diphtheria-patients could be disinfected and fumigated as heretofore, but the placard should remain and isolation continue until the bacteria can no longer be detected.

*As Others See Us.*—It is not given to any but rare and beautiful minds to speak such delightful words of his own profession as those uttered by DR. C. ELLERY STEDMAN, of Dorchester, Mass., in his altogether admirable address to the Norfolk District Medical Society last June, and published in the *Boston Medical and Surgical Journal* of August 22d. The way we are looked at by the public, "the People with a large P," the taunts, deserved and undeserved, we have to bear, the answers we might or should make thereto—these things are touched upon with a combination of wit and pathos, a hand as light and yet as firm as that of the Autocrat's own. We had almost expressed the thought, or the wish, why does he not take up the Autocrat's function? For example, is not this a gem?

"One of our best men, whom all of us respect and many of us love, was called to a young woman in one of his families, and to his vast surprise found the maiden—whom he had known from her birth—in the throes of a miscarriage. After scolding briskly for some minutes, he proceeded to relieve her of her chorionic encumbrances. As he worked red-handed an aunt came in, and rising to the situation, exclaimed, 'So you *are* in the business after all.' The rage of the good-hearted and honorable man may be fancied. He now devotes himself to the infernal gods if he ever attends a woman in that trouble again; and if he finds himself caught at the bedside, he abandons the case."

We regret that we cannot quote the whole address. These are the closing words:

"I believe there are none whose work is of better quality or more conscientiously done than that of the general practitioner, who represents the body of the profession. It ill becomes those whom they serve, to doubt their capacity or to depreciate their work. The doctor may scold, he may even swear when he turns out of his warm bed of a cold night; but he turns out, he plods or drives through the snow and the mud and the cold—too often for no other remuneration than the glow of satisfaction if a knotty diagnosis be cleared up, or if remedies have steered Nature into a safe port. All over the world are men in laboratories, in the dead-house, in the hospitals, in the hovel, striving to wrest her secrets from Nature, satisfied if, after days and nights of toil, they may elucidate one point—forge one link in the chain of evidence that shall establish a truth or expose an error. They live among the dead and dying, in an atmosphere charged with disease and death; and, as Dr. Holmes says, 'They have tasks to perform which the chambermaid and the stable-boy would shrink from undertaking.'

"Yes, we are ignorant; but it is not because the way to knowledge is closed or unfrequented. The old roads are thronged with travellers; and new ones are opened every day, in which one timidly steps at first, another enters and goes farther on the path, more join them, and soon there is a crowd which thickens and presses on. Nature hides her jewels deep; and we must sweat and dig and bore and wash before she will yield her treasure. Much can be told of what has been done, but that is just the line of thought which we avoid to-day.

"To sum up—needless though it be to say it to this company—let us reflect that it is easy to give offence even when we least intend it; that even on the low ground of self-interest we prosper when we keep to the straight path; that in dealing with those not of our own communion, whatever they may do, no one of us can afford for one moment to forget that he is a gentleman. If we are told we do not know much, we confess it with shame and confusion of face; but we know as much, if not more, than those whom we pay for working for us.

We do our best work when no one is watching us; and—thank God!—ours is the only profession that at any time does its work for nothing."

**Yale University Medical School.**—In commenting upon the subject of medical education in the United States in THE NEWS of August 31st, p. 246, we took occasion to say that Yale University was one of the four-year medical schools. We find, however, that we rested under a misapprehension. The corporation has lengthened the medical course to four years, but the change will not take effect until the fall of 1896; in the meantime this year's matriculants may graduate in 1898. The explanation given by the dean for the existing state of affairs is that the corporation of the University could not be assembled in time to take action on the recommendation of the medical faculty to extend the course at once to four years. The reason given seems to us quite inadequate and will not at all redound to the credit of the school. If a university of the standing of Yale allows a mere technicality to stand in the way of higher medical education, how can smaller schools be blamed for holding back.

**"Cellular Altruism" and "Physiologic History."**—For these suggestive and appropriate terms we are indebted to Professor S. H. Gage, of Cornell University, who used them first in his exceptionally able address as President of the American Microscopic Society (*Science*, August 23, 1895). We elsewhere reproduce a few scattered selections from the paper, regretting that want of space prevents us from producing it entire. From a veteran and practical physiologist and microscopist such clear statements show plainly the fast-changing attitude of science as to the more recondite problems of life. With Lombroso acknowledging his defeat, and a "degenerate" follower giving it the *coup de grace* by a personal *reductio ad absurdum*, we trust the anatomic and materialistic position of pseudo-science has reached its inevitable end, and that henceforth physiology and pure induction may be the order of the day.

## SELECTION.

### "PHYSIOLOGIC HISTORY" AND "CELLULAR ALTRUISM."

THERE is another fact in Nature that the microscope has revealed, and that fills the contemplative mind with wonder, and an aspiration to see a little further into the living substance, and so perchance discover the hidden springs of action. This fact may be called *cellular altruism*. In human society the philanthropist and soldier are ready at any time to sacrifice themselves for the race or the nation. With the animals the guards of the flock or herd are equally ready to die in its defence.

Into the veins of the *necterus* was injected some lamp-black mixed with water, a little gum-arabic, and ordinary salt, an entirely non-poisonous mixture. Thousands of particles of carbon were thus introduced into the blood, and could be seen circulating with it through the transparent gills. True to their duty, the white corpuscles in a day or two engulfed the carbon particles, but for several days more the leukocytes could be seen circulating with

the bloodstream, and carrying their load of coal with them. Gradually the carbon-laden corpuscles disappeared, and only the ordinary carbon-free ones remained. Where had the carbon been left? Had it been simply deposited somewhere in the system? The tissues were fixed and serial sections made. The natural pigment was bleached with hydrogen dioxide, so that if any carbon was present it would show unmistakably. With the exception of the spleen, no carbon appeared in the tissues, but in many places the carbon-laden leukocytes were found. In mucous cavities and on mucous surfaces, and on the surface of the skin, were many of them; in the walls of organs were many more apparently on their way to the surface with the load—that is, the carbon is naturally carried out of the tissues upon the free surfaces of the skin and muous membranes, where, being outside of the body, it could no more interfere in any way with it. But what was the fate of the leukocytes that carried the lampblack out of the tissues? They carry their load out and free the body, but they themselves perish. They sacrifice themselves for the rest of the body as surely as ever did soldier or philanthropist for the betterment or the preservation of the State.

In brief, it seems to me that the present state of physical and physiological knowledge warrants the assumption, the working-hypothesis, that life is a form of energy different from those considered in the domain of physics and chemistry. This form of energy is the last to appear upon our planet, last because more conditions were necessary for its manifestations. It, like the other forms of energy, requires a material vehicle through which to act, but the results produced by it are vastly more complex. Like the other energies of Nature, it does not act alone. It acts with the energies of the physicist, but as the master; and under its influence the manifestations pass infinitely beyond the point where for the ordinary energies of Nature it is written "thus far and no further."

It can be stated without fear of refutation that every physiological investigation shows with accumulating emphasis that the manifestations of living matter are not explicable with only the forces of dead matter, and the more profound the knowledge of the investigator the more certain is the testimony that the life-energy is not a mere name. And, strange to say, the physicist and chemist are most emphatic in declaring that life is an energy outside their domain.

The sure way, it seems to me, is the study of structure and function together; the function or activity serving as a clue and stimulus to the investigator for finding the mechanism through which function is manifested, and thus give due significance to structural details which, without the hint from the function, might pass unnoticed.

This kind of microscopical study, it seems to me, may be well designated as *Physiological History*. It is in sharp contrast with ordinary histology, in which too often the investigator knows nothing of the age, state of digestion, or of fasting, nervous activity, rest or exhaustion. Indeed, in many cases it is a source of congratulation if he knows even the name of the animal from which the tissue is derived. Such haphazard observation has not in the past, and is not likely in the



future, to lead to splendid results. If structure, as I most firmly believe, is the material expression of function, and the sole purpose of the structure is to form the vehicle of some physiological action, then the structure can be truly understood only when studied in action or fixed and studied in the various phases of action.

Indeed, if one looks only for form or morphology in the study of histology, the very pith and marrow is more likely to be lost.

[Professor Simon Henry Gage, Presidential Address, American Microscopic Society.—*Science*, August 23, 1895.]

## SOCIETY PROCEEDINGS.

### MISSISSIPPI VALLEY MEDICAL ASSOCIATION.

*Twenty-first Annual Meeting, held in Detroit, September 3, 4, 5, and 6, 1895.*

(Continued from page 336.)

DR. GEORGE W. CALE, of St. Louis, read a paper entitled

#### TWO SUCCESSFUL OPERATIONS FOR TRAUMATIC INSANITY.

He pointed out that insanity due to injuries of the head is of rather infrequent occurrence. In 2200 cases of insanity treated by Kiernan, 45 were of traumatic origin; while Hays records 61 due to the same cause out of 2500 cases. Schlager reports 500 cases of insanity due to concussion of the brain.

The case was reported of a carpenter, twenty-six years of age, with a good family history. No case of insanity or of serious nervous disease had ever been present in the family. In June, 1885, the patient received a blow from a club in the hands of a negro, the wound inflicted being midway between the fissure of Rolando and the external occipital protuberance, slightly to the left of the median line. It was treated as an ordinary scalp-wound, as it suppurated for three months. Four years after the inception of the wound the man complained of severe pain in the left parietal and occipital regions. In October, 1888, he was sent to an insane-asylum, where he remained sixteen months. His condition improved, he returned home, but he was again sent back to the asylum, where he was detained for five months. Four months later he was brought to Dr. Cale, who diagnosed traumatic insanity, and advised operation, which was consented to. Recovery ensued.

The second case was one of acute mania following a trauma. In this also operation was performed and recovery ensued.

DR. WILLIAM FULLER, of Grand Rapids, Mich., related that he had operated in two cases of insanity due to traumatism, one in Montreal in 1870, after the patient had been discharged from an insane-asylum as incurable. When the dura mater was opened some serum escaped. A few days subsequent to the operation the patient was rational, but as soon as the wound closed the hallucinations returned. The brain was then punctured in two or three different directions, but with no result except to establish drainage. The man died two months later, and a post-mortem examination revealed a tuberculous abscess in the fissure of Sylvius contain-

ing about two teaspoonfuls of pus. The other case was due to syphilis, and was relieved by the removal of pressure due to that disease.

DR. J. FRANK, of Chicago, reported an interesting case of insanity in which he trephined, removing a large piece of bone. A piece of the brain, excised for examination, seemed to be healthy. The patient, after being in an insane-asylum for five years, made a complete recovery and took up the thread of life where she had left it. She continued in this way for one year, then relapsed. What was the reason of her recovery? In opening the dura there was a gush of cerebral fluid. Dr. Frank firmly believes that it is not the depression of the skull that produces the bad results, but that at the time of the injury there is a chronic inflammation of the meninges, or a slow inflammatory process, which throws out cerebral fluid. This fluid may be in the ventricles between the dura and the brain or in the tissues of the brain itself, and the theory of concussion held by the profession Dr. Frank believes to be erroneous. He has repeatedly made experiments on dogs by rapping them on the head and rendering them insensible, and immediately thereafter effusion was found.

DR. MAAS, of Detroit, stated that we are in the dark as to the exact cause of the insanity in many cases, and that there is evidently some vasomotor disturbance.

DR. F. MAAS, of Detroit, read a paper, which was largely a statistical one, in which he pointed out the comparative value of the medical and surgical treatment of appendicitis.

DR. WILLIAM E. WIRT, of Cleveland, read a paper on THE USE OF DRY HEAT OF HIGH TEMPERATURE IN THE TREATMENT OF JOINT-DISEASES.

He cited a case of rheumatoid arthritis in which he resorted to this method of treatment. Great improvement in the motion of the joint and in the comfort felt by the patient followed. He also reported a case of rheumatism of a year's standing in which there was more or less fixation. He broke up the adhesions, made use of dry heat at a high temperature, up to 290°, and increased ease with which the patient could move the joint and relief of pain followed.

DR. CARTER S. COLE, of New York, read a paper entitled

#### ULCERS OF THE LEG; ALL CAN BE CURED.

Constitutional conditions that favor morbid states, or that return to a healthy state, should receive proper treatment, whether or not ulcers exist. For systematic purposes ulcers of the leg were designated, according to their appearance, as healthy, irritable, indolent, etc. In intractable cases thorough washing with soap and water and good scrubbing with a stiff bristle hairbrush were conceded first place. If the ulcer be inflamed, irritable, or painful, anesthesia may be required for this and subsequent steps. The next step is a thorough cleaning out of all soft granulations and the base of the ulcer with a sharp curet. The edges of the ulcer are freed from their attachment, and in many cases with a curved sharp bistoury the circumference is nicked at intervals of about one-quarter of an inch. If much hemorrhage follows, a pad of gauze wrung out of a 2 per cent. solution of carbolic acid is placed over the wound and a firm compressive bandage applied from the toes to the knee, the wound

having previously been thoroughly cleansed with the carbolic solution. The dressing, when used, is allowed to remain for twenty-four or forty-eight hours, after which the ulcer will have become a simple one, and be amenable to treatment as follows: No further lotion is used. The wound is wiped with dry cotton, and over and completely covering it are placed strips of diachylon-plaster to protect the ulcer. Over the surgeon's plaster is applied a pad of sterilized gauze, held in place by strips of rubber adhesive plaster, or often simply by a bandage. Then a firm muslin bandage is applied from the toes to the knee, making equable compression. Bandaging should be carefully done. Sometimes two bandages three inches wide and eight yards long are used. The bandage is not removed unless it becomes loose or saturated with the discharge, or the leg becomes painful. In redressing the ulcer dry absorbent cotton is again used to cleanse the wound, and the steps detailed are repeated. Often after two or three dressings the bandage may remain from five to seven days without being disturbed. In some cases a thin scum forms on the ulcer, which must be removed by going over the surface lightly with a curet. With this treatment, in ordinary cases, about three weeks will suffice for the cure of an ulcer of even a dozen years' standing. In extraordinary cases as long as six weeks may be necessary.

DR. EUGENE FULLER, of New York, read a paper entitled

#### HOW TO DIAGNOSTICATE SEXUAL DERANGEMENTS IN THE MALE.

He dwelt on the fact that in the majority of instances sexual derangements in the male are caused by pathologic processes in or about the seminal vesicles, and, further, that they accomplish their results by interfering with the mechanism of ejaculation. He called attention also to the fact that this side of the question has been almost wholly neglected by preceding writers on sexual disorders, who have devoted themselves largely to psychologic causes in this connection, the result being that the great majority of the profession associate sexual disturbances with some radical mental defect. Sexual derangements in the male should be diagnostically arranged in four classes: (1) Those dependent on seminal vesiculitis. (2) Those dependent on neuroses. (3) Those dependent upon primary mental disease or degeneration. (4) Those dependent on general malnutrition and debility. The order of this classification corresponds to the frequency with which these different forms of disease are encountered in practice.

When seminal vesiculitis exists there is generally a previous history of urethral or vesical inflammation, sexual abuse, and the like, all of which are agents tending to produce localized inflammation in the seminal vesicles.

The causes of the second class either inhibit or excite the sexual center by means of reflex nervous action.

The third class of causes includes the different forms of paranoia, in which the sexual sense exists in a perverted form.

The fourth class of causes is a small one. It includes individuals, generally young or middle-aged, who make complaint that they are capable of little sexual exertion, and that feelings of prostration and exhaustion result whenever coitus is attempted.

Reference was made to the different appearances that the varying grades of seminal vesiculitis present to the sense of touch, and attention was directed to the fact that in cases of extensive perivesiculitis involving both sacs an inexperienced examiner is liable to err in diagnosis, mistaking the condition for hypertrophy or inflammation of the prostate. It was pointed out that to become perfected in the feel of the seminal vesicles the finger needs as much practice as does that of the gynecologist in palpating the ovaries and tubes. To obtain the necessary practice the genito-urinary surgeon was advised to examine habitually in this manner every male case coming into the clinic until all normal and pathologic conditions can be fully appreciated.

DR. S. P. COLLINGS, of Hot Springs, Ark., read a paper on

#### CHRONIC SEMINAL VESICULITIS WITH HEMORRHAGE.

He said that the usual cause of this trouble is the extension of gonorrheal inflammation from the prostatic urethra through the ejaculatory duct into the vesicle itself; at least there is usually a history of a former gonorrhea, with a chronic deep urethral trouble remaining. The vesicles are sometimes involved in very acute and severe gonorrheal inflammation, with or without the implication of the cord and epididymis. They may also be involved in tuberculous inflammation, although practically never primarily. A case has been reported in which after death the vesicle was found to be primarily involved. The most important symptom is the disturbance of the sexual function. The character and appearance of the seminal fluid are more or less changed. Its consistency is so increased at times that it is gelatinous. The diagnosis of subacute or chronic seminal vesiculitis would be difficult were dependence placed entirely upon the symptoms in reaching a conclusion. They are vague and, at times, misleading, except the appearance of blood in the seminal fluid, which, if thoroughly admixed, would at once indicate a diseased condition of one or both vesicles. It was recommended that if inflammation of the vesicles occurs in one whose urethra is strictured, the stricture must be cured before hope can be entertained of permanent results in vesical treatment.

DR. C. T. DRENNEN, of Hot Springs, Ark., read a paper on

#### SYPHILIS AND ITS TREATMENT.

He spoke of the treatment of this disease in connection with the use and abuse of certain so-called anti-syphilitic remedies. He said that from the most recent researches it is not possible as yet to form definite conclusions as to the usefulness of sero-therapy in the treatment of syphilis. But the reports are quite encouraging, and there is hardly a doubt that it possesses value in at least the tertiary lesions. Fournier believes the good effect to be due not to any specific, but to its rehabilitat- ing influence upon the system.

It was pointed out that the hot water at Hot Springs was well known to exert an influence peculiar in its effect for good, and this resort constituted therefore a veritable Mecca for syphilitics; that the action of the water is eliminative, stimulating, and antiseptic, and that larger doses of mercury and potassium iodid can be given without harm, have been demonstrated by experience

and observation; that its exact or specific action is unknown, and that its value is attested to by the multiplied thousands throughout the country are propositions that are incontrovertible.

DR. WILLIAM T. BELFIELD, of Chicago, described and presented an instrument for the purpose of securing asepsis in operations upon the bladder and prostate.

DR. THOMAS H. MANLEY, of New York, read a paper entitled

#### THE ELEMENT OF VASCULAR COMPRESSION IN FRACTURE-TREATMENT.

He stated that the extent of damage borne by the vessels in a given case of fracture will primarily depend upon the degree and quality of force applied and the line of treatment adopted. In a series of experiments made during the past year on the blood and blood-vessels, under a multiplicity of conditions, in the lower animals under anesthetics, one question that Dr. Manley took pains to determine definitely was whether, as a general rule, the circulation of the distal part of the limb was retarded in cases of fracture. Without entering into detail as to the great diversity of vascular phenomena observed in a frog's webbing under the microscope after single, multiple, and compound fractures were produced, it will suffice to say that with but few exceptions, immediately and for a considerable period of time after the bone was broken, the circulation in the capillaries and the smaller arteries was completely arrested. In several it was found that for several days all of the smaller vessels were motionless, and in a few they so remained until the fractured ends of the bones united. These experiments were extended to the mammalia—the shafts of the limbs of pups, kittens, and adult dogs, of different ages and of various sizes.

DR. H. O. PANTZER, of Indianapolis, Ind., read a paper entitled

#### THE SIGNIFICANCE OF FISSURE-FRACTURES OF THE ARTICULAR ENDS OF THE LONG BONES.

He reported a case of fissure-fracture involving the outer third of the head of the radius, having loose attachments to its body. Excessive callus had developed without effecting union. The splinter and the callus were removed, when reposition of the joint was easily attained. The crepitus felt at the first examination and the failure to find it again, it was thought, should have suggested fissure-fracture. When the first dressing was removed, the limb seemed to present a perfect condition and to promise full restoration. These prospects were changed as soon as passive and active mobilization was begun. The probability is that the attempts at mobilization for this kind of injury were made too soon, and that they were at least partially responsible for the subsequent unfavorable developments. The conclusion was reached that when undue painfulness and subsequent swelling, with no gain or even a loss of mobility, attend the efforts at mobilization after kindred lesions, further efforts at mobilization should be deferred. The possibility of a fissure-fracture should be considered in determining the diagnosis and treatment of all cases of joint-injury.

DR. E. W. ANDREWS, of Chicago, contributed a paper entitled

#### IMBRICATION OR LAP-JOINT METHOD—A PLASTIC OPERATION FOR HERNIA.

He maintained that the principle of imbrication or overlapping the several aponeurotic layers of the abdominal wall may also enter into other abdominal operations to advantage. The present paper referred only to its use in inguinal hernia. Here it is to be employed to supplement and reinforce existing methods, without losing sight of their good qualities. While based upon the best modern, open method, and while confessedly an outgrowth of experiences with the Macewen, Bassini, Halsted, and similar operations, yet the carrying out of the imbrication-idea so far changes the technic as to make it as different from them all as they are from each other.

The interlocking or overlapping principle of uniting the musculo-aponeurotic layers of the abdomen has been an outgrowth of clinical experience with Dr. Andrews rather than of theory. He adopted it at first as an expedient in cases in which the Bassini operation seemed difficult and needed supplementing, but of late the value of the principle had seemed to him more and more evident, and he had used it in all his cases. The following conclusions were presented: Any successful method of radical cure of hernia must be a true plastic operation upon the musculo-aponeurotic layers of the abdominal wall. Cicatricial tissue and peritoneal exudates are of no permanent value; a large, strong flap is needed, of sufficient size to fill the internal ring, together with triplicate layers of aponeuroses; interlocking of the layers, giving broad surfaces of union; shortening of the anterior as well as the posterior wall of the canal, making them mutually supporting, and relieving tension on deep sutures; and ample protection of the cord.

DR. CHARLES A. L. REED, of Cincinnati, read a paper entitled

#### FURTHER OBSERVATIONS ON THE RADICAL CURE OF RUPTURE BY THE INTRAPELVIC METHOD, WITH ILLUSTRATIVE CASES.

The method described differs from the several methods of radical operation now in vogue; differs, in the first instance, in the fact that it is intrapelvic, while the others—Bassini's, Halsted's, McBurney's, Macewen's—are extrapelvic.

The essential point of differences between Dr. Reed's operation and that of Bassini's consists (1) in leaving undisturbed the extremely tense fascia composing the anterior wall of the ring; (2) in leaving the cord in the position that Nature designed for it, entirely protected by the normal fascia; (3) in closing the internal ring on the inside of the pelvis and in protecting it by a strong peritoneal pad; (4) in avoiding the menace to virility arising from a transplantation of the cord, its possible constriction by an artificial ring of tense fascia, and its necessary investment by an inflammatory exudate; (5) in increasing the resistance of the parts by fortifying the fascia comprising the anterior wall of the ring, and by increasing and making permanent the obliquity of the cord within its normal canal.

The advantages of the operation consist in securing, by the unfolding of the redundant but attenuated median fascia, the formation of a column that, when consoli-



dated by inflammatory adhesions, has a certain retentive and supportive power, the influence of which is of value in preventing recurrence. The anchorage of the recti in the median line destroys the retentive power of the wall.

DR. HENRY O. MARCY, of Boston, followed with a paper entitled

#### SURGICAL TREATMENT OF HEMORRHOIDS.

This paper was in the nature of a reply to the strictures cast upon the Whitehead operation by Dr. Edmund W. Andrews, of Chicago, in a paper read before the Illinois State Medical Society. Dr. Marcy believes that if, in the statistics given by Dr. Andrews, the names of the operators were mentioned, most of the disastrous results would be found to have followed the work of incompetent men. His results have been excellent in those cases in which he has performed the Whitehead operation, slightly modified by himself.

DR. JOHN RIDLON, of Chicago, read a paper on

#### CLUB-FOOT,

which he defined as a distortion of the foot in its relation to the leg. The simple and compound varieties of club-foot were dealt with. Nearly all of the congenital cases present the compound variety—equino-varus; a few present equino-valgus; rarely there is seen a simple calcaneus. Of simple equinus Dr. Ridlon had met with but a single instance, and of simple varus and simple valgus he had seen none. Of the acquired variety, simple equinus is by far the most frequently found; next in frequency is simple valgus, then equino-varus, calcaneo-valgus, equino-valgus, simple varus, and varus in one foot and valgus in the other. The acquired deformity occurs more than three times as frequently as the congenital form. The etiology of the congenital forms was next dealt with, as well as that of the acquired forms. Then followed the symptoms, diagnosis, and prognosis.

The treatment of club-foot is mechanical or operative, or both mechanical and operative. Both these measures were dealt with at length.

DR. E. E. TULL, of New York, read a paper on

#### VAGINAL CASTRATION,

in which he claimed for this method a lower mortality, a shorter convalescence, and a wider adaptability, as it may be employed in cases too weak for abdominal section.

DR. F. G. GRONER, of Grand Rapids, Michigan, read a paper entitled

#### A NEW PHASE OF CELIOTOMY.

He related a suit for malpractice which had just been terminated in Grand Rapids. The suit was commenced against a doctor some time ago, and was for \$50,000. The defendant died, but the court held that the cause of action survived, and that the suit could be brought against the estate. The jury returned a verdict for \$10,000. It was contended that there was no malpractice, and fault was found with the laws that permitted a suit against a doctor to survive his death, and be a menace to his widow and children. It was thought a remedy should be provided by the Legislature.

DR. J. FRANK, of Chicago, read a paper entitled

#### RESULTS OF FIVE YEARS' EXPERIENCE WITH INTRA-ABDOMINAL SHORTENING OF THE ROUND LIGAMENTS.

He related that since November, 1889, he had had the opportunity of performing this operation seventeen times, with only one failure, and without any deaths. In these cases the operation was performed for retroversion, prolapsus, and retroversion with prolapsus of the uterus.

The median incision is made a trifle lower than in ordinary celiotomy. The round ligament is caught up anteriorly with a sharp or a blunt hook, and is then held taut by an assistant. A small, full-curved needle threaded with fine silk is then passed through the loop of the round ligament and is brought back in the reverse manner through the other half of the loop. No portion of the broad ligament is included in any of the sutures. Too much stress cannot be placed upon this particular procedure of passing the needle through a part of the cord and not around it, for in passing the needle around the cord there is danger of strangulation, as the blood-supply and the nerve-supply would be thus entirely shut off.

About fifty per cent. of the cases have remained under observation since the time of operation, which, in some instances, has been as long as two years, and in all of these the uterus retains its corrected position. This operation should be performed in preference to any other in all cases in which the uterus is prolapsed or immediately falls back upon replacing it with a uterine sound, and in which pessaries and tampons afford no relief, clearly showing that there must be some force that does not permit the uterus to remain in its normal position.

DR. THEODORE A. MCGRAW, of Detroit, delivered the Address on Surgery. He selected for his subject

#### THE PRESENT STATE OF OUR KNOWLEDGE OF CARCINOMATA AND OTHER TUMORS.

He said that a comparison of the ideas that prevailed relative to these diseases thirty years ago with those the profession holds to-day would show a change that marks rather a clearer view of the nature of the problem than any actual gain in its solution. Before Virchow the subject of tumors and carcinomata was wrapped in greatest confusion. Tumors were divided and classified in that era according to their clinical histories. They were divided into the innocent, which were looked upon as purely local, and the malignant, which were regarded as due to dyscrasie, the seat of which is in the blood. Under the influence of the new cellular pathology faith in so-called dyscrasie was abandoned and supplanted by a new doctrine, which taught that tumors and carcinomata resulted ever and always from aberrations in cellular nutrition, development and growth. It may be said that during this time the energies of the profession, as far as tumors are concerned, have been confined almost solely to this channel. The most successful attempt to account for the origin of tumors and carcinomata was that of Cohnheim.

The arguments offered in defence of the parasitic theory of carcinomata are (1) the microscopic evidence; (2) the frequent occurrence of auto-inoculation, and the evidently infectious course of the disease in the human

body; (3) the endemic occurrence of carcinomata in certain localities, and in rare instances in the same houses; (4) occasional successful inoculation of the disease in animals. The arguments against it are the many instances of hereditary tendency; the general failure of experiments of inoculation, and the fact that metastases occur, not through the transfer from one point to another of parasitic germs, as is usual in parasitic diseases, but by the transmigration of cells bearing the same characters as the cells of the original tumor, and the subsequent proliferation of the migrating cells; the fact that not only carcinomata but tumors of all kinds have a greater or less tendency to produce metastases; the tendency exhibited by embryonic remains and histoid tumors to become malignant.

Dr. McGraw closed by saying that physicians should be better instructed in the means of diagnosis and in the necessity of early operative treatment. And last, but not least, the laity could be induced to assist, not only by liberal contributions of means, but by that intelligent co-operation which would lessen the difficulty of collecting evidence and making post-mortem examinations and keeping the sufferers out of the hands of the quacks.

(To be concluded.)

#### AMERICAN ORTHOPEDIC ASSOCIATION.

*Ninth Annual Meeting, Held in Chicago,  
September 17, 18, and 19, 1895.*

THE Society was called to order by the President, DR. JOHN RIDLON, of Chicago.

After the transaction of some routine business, the President delivered his address. He selected for his subject

#### THE GROWTH AND PROSPERITY OF THE AMERICAN ORTHOPEDIC ASSOCIATION.

He said, if one thing more than another had been instrumental in bringing about the present healthful organization, it was a wisely framed Constitution and By-Laws and a strict adherence to their requirements. To become a member the candidate must be personally known by at least two members of the Association, who voluntarily nominate him for membership, and he must have a creditable record of orthopedic work either as a writer, a teacher, or a clinician.

The generous expenditure of practically the entire income of the Association in the annual publication of its volume of *Transactions* has played no unimportant part in the development of the Society. In this regard there was urged upon the members the serious consideration of the publication of a monthly or, at least, a bi-monthly journal, which shall contain all the papers read and discussions held before the Association and abstracts of all other worthy publications upon orthopedic subjects, whether first appearing here or abroad. Such a journal could be issued at little, if any, increase upon the cost of the present *Transactions*, and in the hands of an enterprising editor could within a year or two be made almost self-supporting.

The address was closed with a tribute to the memory of Drs. Little and Detmold.

DRS. R. W. LOVETT and JOHN DANE, of Boston, contributed a joint paper entitled

#### FLAT FOOT.

They submitted the following conclusions: 1. That the feet of the infant at birth are not flat; that a tracing made at that time resembles that of the normal adult foot. 2. That a body of fat develops under the arch which gives the appearance of flat foot for some years, and that at the age of four or five years this is absorbed. 3. That the carbon-tracing is not a perfect method of studying abnormalities of the arch of the foot, because it fails to detect the slighter abnormalities or to record pronation. 4. That the element of pronation is more constant than breaking down of the arch of the foot and may be entirely separated from the latter. 5. That the condition of pronated foot without breaking down of the arch of the foot should be recognized and not confused with flat foot. 6. That the treatment of pronated and of flat foot is the same, and consists in the use of proper boots, the application of a pad or plate, the stretching of the gastrocnemius muscle when it is shortened, and in the routine use of massage, and always of exercises to develop the muscles that hold up the arch.

DR. S. L. MCCURDY, of Pittsburgh, read a paper on

#### CONGENITAL ABSENCE OF THE RADII, WITH OPERATION.

He detailed the case of an infant five months old, born at full term after a normal presentation, presenting complete absence of the radii, with abnormal relationship between the bloodvessels and the soft structures of the forearm. Otherwise the child was perfectly developed. Operative interference was at first not advised, for the reason that no successful method had been invented. In the worst cases, in which there is much bony deficiency, the choice lies between amputation and doing nothing. Bardenheuer two years ago proposed a new method for the operative treatment of these deformities. His operation on the forearm consisted in replacing the defect at its lower part with bone, and thereby permanently correcting the deformed position of the hand.

By a longitudinal incision the distal end of the ulna and the carpus are exposed, and the first isolated from its attachments. The ulna is then split through its middle into a radial and an ulnar section. These are separated by allowing the carpal bones to come up between them. By means of a nail passed through each side the ends are fixed to the carpus. A plaster bandage is applied, and left on for four or five weeks. This operation is easily carried out. It has been performed by Bardenheuer three times, and the results in each instance were good, from both a functional and a cosmetic point of view. In all, the deformity was permanently corrected. The same principles may be adopted in the treatment of other defects. In cases of congenital defects of the tibia or fibula the same operation has been performed once for each variety. The best result was obtained with the fibular defect. In the case reported, operation was eventually undertaken, the desire being to adhere to the method described as nearly as possible. It was not possible, however, to follow the method. The soft structures, tendons, etc., were so much shortened that to attempt to shift the end of the ulna across to the center of the carpus would have been impossible except by a virtual amputation of the arm. To have split the ulna and wire the carpal bones between these ends

would have been impossible. The only method was to sever the ulna at a point where the free end of the upper fragment could be brought to the semilunar bone. The semilunar was curetted and drilled, and after drilling the ulna the two bones were adjusted with silkworm-gut. It was necessary to divide the tendons upon the radial side of the forearm before the hand could be straightened. A number of arteries were severed and required ligation. The cut through the soft tissues was made obliquely across the forearm, beginning upon the dorsum of the wrist and passing upward and around under the forearm, the object being to allow the structures to slide upon each other. These were then sutured in the corrected position, thus avoiding the gap that would otherwise have been left after a cross-section. After dressings were applied the hand was put up in plaster-of-Paris. The wound healed by primary union. The operation was first performed on the left hand, and a second operation was planned for the right arm, but the patient died from pulmonary tuberculosis five months afterward.

A case was also reported of congenital deformity, with a decided history of a maternal impression. A woman, whose husband was a railroad-conductor, was startled, when about six weeks pregnant, by the appearance of a crowd of men carrying on a stretcher a man, whom she thought was her husband. She had a habit when startled or in grief of grasping the fingers of the left hand, including the thumb just below the knuckles, with the right hand. While the injured man was not her husband, the impression was imparted to her unborn child, and manifested itself in the peculiar attitude of the hand.

DR. A. M. PHELPS, of New York, reported a case of congenital dislocation of the shoulder-joint, and described an operation for its relief.

DR. JAMES E. MOORE, of Minneapolis, read a paper on

#### REMOVAL OF THE ASTRAGALUS FOR TALIPES VALGUS.

He said that the modern non-operative treatment of talipes valgus was so satisfactory that it is rarely necessary to consider operative measures. In acquired flat foot, several operations have been tried in extreme cases, and among them Ogston's, which has probably been performed more frequently than any other, although at present it does not seem to have any enthusiastic advocates. The scaphoid has been removed, but the operation has not been followed by flattering results. The case was reported of a woman, twenty years of age, who had cut the internal lateral ligament of the ankle when she was a child by stepping on a piece of glass. An examination showed that the uninjured foot manifested a natural tendency toward flat foot, and the injured member showed extreme flat foot, with the inner side of the great toe turned almost directly downward. The condition had grown gradually worse for some years, so that when the patient came under observation she had been obliged to give up her employment as a domestic on account of the severe pain produced by standing or walking. Chloroform was administered, with the hope of forcibly correcting the deformity. Owing to the extreme rigidity of the foot, failure was feared; so the patient's consent was gained

to resort to a cutting-operation if it should be necessary. It was found impossible to overcome the deformity by manual force, and an operation was decided upon. The displaced astragalus formed the bulk of the deformity, and was the keynote of the trouble. This was removed through a longitudinal incision on the inner side of the foot, which could be immediately placed in a very satisfactory position. The usual surgical dressing was applied, and the foot held in proper position by a plaster-of-Paris bandage. The healing was prompt and satisfactory, and in about two months the patient began to walk. Marked improvement in the position of the foot followed. About one year after the patient began to walk the ankle, which was atrophied, had developed considerably. The result of the operation was more satisfactory from a practical than from an esthetic point of view. The patient still complains of some pain. When she stands much or walks more than usual the foot is somewhat swollen in the evening, but she is now at work. While she is in every way better than before the operation the result is not perfect, but it is hoped that eventually she will be quite well.

DR. JOEL E. GOLDTHWAIT, of Boston, read a paper entitled

#### TENDON-TRANSPLANTATION IN THE TREATMENT OF DEFORMITIES RESULTING FROM INFANTILE PARALYSIS.

Attention was called to the possibility of furnishing better mechanical attachments for certain non-paralyzed or only partially paralyzed muscles as a part of the treatment of infantile paralysis. A certain number of cases were reported with results that were most gratifying, and from which it appears not unreasonable to class tendon-transplantation with tenotomy and the other surgical procedures that have a place in the treatment of this condition. At the knee undoubtedly tendon-transplantation can be employed to advantage in a certain limited number of cases, and possibly at some of the other articulations, although the best results are to be looked for in those portions of the body where the tendons are well formed and lie superficially. The operation is not to be attempted upon the tendons of muscles that are wholly paralyzed, except possibly in rare instances, for the purpose of furnishing additional support of a purely tendinous character. It is conceivable that in the foot, for instance, if the tendons on the inner side are more atrophied and offer less resistance than those on the outer side, these tendons might be attached to the inner side, and thus increase the lateral support. This is merely a suggestion, however, not having been tried practically, and the benefit to be derived therefrom would undoubtedly be slight. There is also a question as to whether or not two atrophied tendons would unite firmly, although from watching the other cases it is doubtful if there would be any trouble on this account.

The best results from tendon-transplantation are to be obtained in cases in which one group of muscles has been destroyed, leaving the antagonizing or accessory muscles very little, if at all, impaired. Such a condition results in a definite deformity, which becomes more marked as age increases. The best illustration of this is to be found in the condition so commonly seen in the



foot when the gastrocnemius and the muscles at the inner side of the foot have been destroyed, the peroneal muscles retaining their normal contractility. The result is a valgus which constantly increases, both from the muscular contraction and from the faulty mechanical position in which the weight of the body must be received upon the foot in walking or in standing. The same result is also seen in connection with the anterior muscles of the foot; the anterior tibial and the extensor pollicis so often being destroyed, the peroneus tertius and the extensor communis digitorum are the only muscles left for flexing the foot at the ankle. This also results in a valgus which steadily increases.

To correct these mechanical conditions and to use the non-paralyzed muscles to the best advantage, Dr. Goldthwait has operated upon four cases, providing new attachments for the tendons of the muscles.

The first case was in a young woman, nineteen years of age, who complained of trouble in the left foot. When nine months old she became paralyzed, the palsy partially clearing up, and leaving the foot very weak, so that walking had always been difficult. The difficulty had been growing worse and there had been considerable pain referred to the inner side of the foot under the inner malleolus. The left leg was an inch shorter than the right. There was marked atrophy of the left calf, and the foot was in the position of extreme calcaneo-valgus. In walking the weight was borne upon the inner edge of the heel, the anterior portion of the foot being turned outward at an angle of fully 40° with the normal axis of the ankle-joint. The posterior muscles were all paralyzed with the exception of the peroneus longus and brevis, and the tendons of these muscles, instead of lying in the groove behind the external malleolus, had been drawn forward, so that they rested on the outer surface of the malleolus near its anterior edge. With the tendons in this position, the action of the muscles caused extreme valgus and slight flexion at the ankle, instead of extension, as should be the case. The anterior muscles were practically normal, and, having no posterior muscles to antagonize them, their contraction caused such extreme flexion that the dorsum of the foot rested against the anterior surface of the leg.

An oblique incision four inches long was made, so that it crossed the tendo Achillis about an inch above its insertion into the os calcis. Through this wound the peroneal tendons were exposed and divided at the lower edge of the malleolus. The tendo Achillis was then freed and the tendon of the peroneus brevis passed under this and attached to the tendon of the flexor longus pollicis. The tendon of the peroneus longus was then attached to the tendo Achillis, after which the wound was closed and a plaster-of-Paris bandage applied, holding the foot extended in order to relieve the strain as much as possible from the sutured tendons. One month later a valgus-plate was applied, and this has been worn since.

The method of attaching the tendons to each other is of great importance, and unless they are firmly joined the benefit of the operation is lost. The tendon to which the attachment is to be made is split and the end of the severed tendon is drawn through this slit, and securely held by two quilted sutures, which are so placed that when tightened the outer tendon is spread out, furnishing a broad surface for union. In the case

reported the valgus has largely been corrected, and what remains is controlled by the plate, so that the tread of the foot in walking is quite normal. As a fair test of the result the patient has during the last five months been doing general housework and has suffered no inconvenience from the foot.

DR. ROYAL WHITMAN, of New York, read a paper entitled

A STUDY OF THE WEAK FOOT WITH REFERENCE TO ITS CAUSES, ITS DIAGNOSIS, AND ITS CURE; WITH AN ANALYSIS OF ONE-THOUSAND CASES OF SO-CALLED FLAT FOOT.

He emphasized the importance of looking on the foot as a mechanism, and on the flat foot not as a deformity only, but rather as a weak or disabled machine. Attention was called to the fact that the normal foot is not only a support, but also a lever. If this activity or leverage be diseased or lost, the foot will be subjected to great mechanical disadvantage. Particular stress was laid upon the component elements which made up the weak or flat foot. These are (1) the improper distribution of the weight upon the foot because of the pronation or valgus; (2) the displacement of the line of strain by the abduction of the foot; and (3) secondary lowering of the arch.

The various predisposing and exciting causes of weakness were enumerated. The importance of the early recognition of the weakness and of the training of children, in order that future disability might be avoided; the importance of the proper shoe, in both childhood and adult life; and the treatment for the various grades of weakness and deformity were then discussed and illustrated by diagrams and casts.

In conclusion, the following points were emphasized: Flat-foot, in its surgical sense, is a compound deformity, of which the elements of valgus and abduction, the improper distribution of the weight and strain, are of vastly greater importance than the depth of the arch. The weak and flat foot can be cured, but only by the application of the simple principles that any mechanic would apply to a disabled machine whose structure and use were known to him; in other words, there can be no permanent cure of weakness and deformity unless normal function is regained, nor effective treatment unless it has this end in view. The term weak foot has at least this advantage, that it implies nothing that the student must unlearn; it is because of the misapplication and misapprehension of flat foot, and because of the associations that have so long obscured the rational treatment of the deformity, that the term is to be discarded.

DR. L. A. WEIGEL, of Rochester, N. Y., contributed a paper on

METATARSALGIA.

DR. ROSWELL PARK, of Buffalo, N. Y., read a paper on THE DEFORMITIES AND MALFORMATIONS RESULTING FROM ACUTE INFECTIONS IN BONE.

He said that clinically there are three locations in which bone-infections are most commonly met with—beneath the periosteum, in the epiphyses, and in the diaphyses. From the pathologic side infections are of four main varieties: (1) tuberculous; (2) staphylococcus; (3) streptococcus; (4) pneumococcus. To these may be added rare instances in which other

organisms are primarily or secondarily present. Of these, certainly the more common is the tuberculous form, whose manifestations are usually not acute. The other three may be grouped in a general way as pyogenic forms of invasion in which pus is practically invariably produced, providing that only sufficient time have elapsed. Allusion was made to the acute miliary tuberculosis of bone that corresponds in most essentials with similar invasion of the lungs. This variety is not quite so rapid in course as the pyogenic forms, and it may take two or three weeks to produce such destruction of the bone as to necessitate operation. It stands, therefore, in an intermediate position between the acute infections and the slower (usually the tuberculous) lesions. Nevertheless, it is sufficiently acute to demand prompt recognition, and when recognized it may often be relieved by prompt operative interference. The course of a bone-disease will depend first upon the location of the lesion, and secondly upon its character. In 30 cases observed by Kocher the disease occurred 13 times in the tibia and 11 in the femur. In 98 cases collected by Leucke and Volkmann the femur was involved 36 times, the tibia 34, and the humerus 11. Disease in the immediate neighborhood of the hip-joint is about five times as common as in that of the knee. Comparing epiphyses with diaphyses, and accepting Schede's 67 cases, it is found that of 28 cases in which the femur was involved, in half the disease was in the shaft and half in the diaphysis. Of 27 cases in which the tibia was involved only 9 concerned the epiphysis, and of 7 cases in which the humerus suffered 2 involved the epiphysis.

Necrosis concerns orthopedists mainly in that in many instances it produces a weakening of the bone that may lead to fracture, deformity, or curvature. Dr. Park has observed spontaneous fractures of necrotic bone; in one case a necrotic femur broke as he was lifting the patient upon the operating-table. Incidentally there is danger of carcinoma in some of these cases. Volkmann has collected 32 cases in which old and fistulous passages became the site of epitheliomatous changes, and in which carcinoma was the final result.

(To be concluded.)

## NEWS ITEMS.

*The Vermont State Medical Society* will hold its eighty-second annual meeting at Burlington, October 10 and 11, 1895. The program includes the following communications: Obituary of S. T. Brooks, M.D., by H. S. Browne, St. Johnsbury. Our Profession, by W. D. Huntington, Rochester; discussion, F. D. Robertson, St. Albans. Address in Medicine (Vice-President's Address), by F. F. Chaffee, Hartford; discussion, H. R. Wilder, Swanton. Cholelithiasis, by E. H. Martin, Middlebury; discussion, L. W. Hubbard, Lyndon. Intestinal Indigestion: its Dietetic and Rational Treatment, by William H. Porter, New York City; discussion, J. H. Jackson, Barre. Cases of Tuberculosis Treated with Tuberculin and Antiphrasin, by L. M. Greene, Bethel; discussion, J. S. Richmond, Windsor. Diabetes Mellitus, by F. C. Morgan, Felchville; discussion, G. E. Lane, Ludlow. President's Annual Address, J. H. Linsley, Burlington. Subject: Some Suggestions Concerning the Examination of Blood; discus-

sion, J. H. Jackson, Barre; A. P. Grinnell, Burlington; and E. W. Shipman, Vergennes. Address in State Medicine, by C. S. Caverly, Rutland; discussion, O. W. Sherwin, Woodstock. Medical and Surgical Treatment of Appendicitis, by S. C. Gordon, Portland, Me.; discussion, L. M. Bingham, Burlington. Blood-stains, by J. N. Jenne, St. Albans; discussion, O. H. Pettingill, Saxtons River. The Relations of the Medical Profession to Certain Social Conditions, by Charles H. Cook, Natick, Mass.; discussion, F. R. Stoddard, Shelburne. The Border-lines of Insanity, by S. E. Lawton, Brattleboro; discussion, J. M. Clarke, Burlington. Address in Surgery, by J. B. Wheeler, Burlington; discussion, E. M. Pond, Rutland. The Prostate, Some of its Acute and Chronic Conditions, and their Treatment, by L. Bolton Bangs, New York City; discussion, H. D. Holton, Brattleboro. Treatment of Injuries of the Eye, by C. E. Chandler, Montpelier; discussion, George H. Gorham, Bellows Falls. Heredity, by O. G. Stickney, Barre; discussion, C. C. Smith, Gaysville. The Antitoxin-treatment of Diphtheria, by Edward R. Campbell, Bellows Falls; discussion, J. H. Hamilton, Richford.

*Suicide in Vienna.*—According to returns made by the Central Statistical Commission, the number of suicides in Vienna during 1889-1893 amounted to 1254—3.2 per 10,000 of the population. Among the corresponding returns of some other Continental towns may be mentioned Paris with 3.6, Brussels with 3.29, Berlin with 2.98, Munich with 1.98, and Breslau with 3.91 per 10,000 of population. During the seven preceding quinquennial periods 360 cases of suicide occurred in 1854-1858, 367 in 1859-1863, 565 in 1864-1868, 631 in 1869-1873, 1020 in 1874-1878, 1129 in 1879-1883, and 1196 in 1884-1888. Among 100 cases of suicide 77 were men and 23 were women. Adolescents appear to have every year more frequent recourse to this method of terminating their real or supposed miseries, the highest rate being registered at the age from sixteen to twenty-five years. Among 100 suicides 32.5 ended their days by hanging, 23.5 by pistol-shots, 22.4 by poisoning, and 5.8 by drowning. With respect to the poisons potassium cyanid was resorted to in 12.7 per cent. of the cases, caustic lye in 22.7 per cent., phosphorus in 26.1 per cent., and sulphuric acid in 7.2 per cent. As for seasonal distribution the proportions were: January, 8.5 per cent.; February, 7.6 per cent.; March, 8.9 per cent.; April, 9.1 per cent.; May, 10.5 per cent.; June, 8.7 per cent.; July, 8.1 per cent.; August, 7.2 per cent.; September, 7.4 per cent.; October, 8.3 per cent.; November, 7.8 per cent., and December, 7.9 per cent.—*Lancet*, Sept. 14, 1895, p. 704.

*The Revival of the Index Medicus.*—The following is a list of additional subscriptions for the *Index Medicus* received by Dr. John S. Billings from September 7th to September 24th:

Albany, N. Y.: Dr. Henry Hun.  
Alhambra, Cal.: Dr. Milbank Johnson.  
Ann Arbor, Mich.: Medical Department, University of Michigan.  
Baltimore, Md.: Dr. Thomas S. Cullen.  
Boston, Mass.: Dr. J. W. Elliot, Dr. R. H. Fitz, Dr. M. H. Richardson.  
Cleveland, Ohio: Dr. William H. Humiston.

Colorado Springs, Col.: El Paso County Medical Society.

London, Eng.: British Medical Association.

Louisville, Ky.: Index Medicus Club.

Nashville, Tenn.: Dr. J. H. Mills.

New York City: Dr. Robert Abbe, Dr. S. T. Armstrong, Dr. L. Duncan Bulkley, Dr. D. Bryson Delavan, Dr. Alexander Lambert, Dr. Robert T. Morris, Dr. Frederick Peterson, Dr. R. W. Taylor, Dr. W. Gilman Thompson, Dr. F. H. Wiggan.

Philadelphia, Pa.: Dr. John W. Croskey, Dr. Howard F. Hansell, Dr. John H. Packard.

Syracuse, N. Y.: Dr. Henry L. Elsner, Syracuse Academy of Medicine.

The total number of subscriptions equals 105. The additional 95 should be easy of attainment.

An Army Medical Board will be in session at Washington, D. C., during October, 1895, for the examination of candidates for appointment to the medical corps of the United States Army, to fill existing vacancies.

Persons desiring to present themselves for examination by the board will make application to the Secretary of War, before October 8th, for the necessary invitation, giving the date and place of birth, the place and State of permanent residence, the fact of American citizenship, the name of the medical college from which they were graduated, and a record of service in hospital, if any, from the authorities thereof. The application should be accompanied by certificates based on personal acquaintance, from at least two reputable persons, as to citizenship, character, and habits. The candidate must be between twenty-two and twenty-nine years of age, and a graduate from a regular medical college, as evidence of which his diploma must be submitted to the Board.

Further information regarding the examinations may be obtained by addressing Dr. George M. Sternberg, Surgeon-General, U. S. Army, Washington, D. C.

**The Rush Monument Fund.**—Dr. George H. Rohé, Secretary and Treasurer of the Rush Monument Committee, announces that the sum-total of the Rush Monument Fund to date amounts to \$3357.39. Among the recent contributions are the following:

Dr. Nicholas Senn, Chicago . . . . .	\$100 00
Dr. Andrew Annan, Emmetsburg, Md. . . . .	50 00
Eastern Ohio Medical Association (through Dr. J. C. M. Floyd) . . . . .	10 00
Medical Society, Wayne County, New York (through Dr. D. S. Colvin) . . . . .	10 00
Dr. George M. Sternberg, U. S. A. . . . .	10 00
Dr. J. R. Buist, Nashville, Tenn. . . . .	5 00
Dr. George M. Gould, Phila. . . . .	5 00
Dr. Franklin B. Ferguson, Deer Island, Me. . . . .	2 00
Dr. George N. Acker, Washington, D. C. . . . .	1 00
Dr. John B. Hamilton, Chicago . . . . .	1 00
Dr. W. H. Marsh, Solomons, Md. . . . .	1 00
Dr. Jacob L. Williams, Boston . . . . .	1 00

**Dr. Albert Leary Gibon**, Medical Director of the U. S. Navy, having reached the age of sixty-two years, will be retired from the service on September 28, 1895, after a service of more than forty years, eleven of which were spent at sea and more than twenty-seven on shore. He retires with the rank of Commodore (Brigadier-General).

Dr. Gibon was graduated from the Philadelphia College of Medicine and Surgery in 1852, and was Professor of Chemistry and Toxicology in that institution in 1853-54. He entered the navy in 1855 as Assistant Surgeon, becoming successively Passed Assistant Surgeon in May, 1860; Surgeon in August, 1861; Medical Inspector in November, 1872; and Medical Director in August, 1879. He has represented the medical department of the navy in many important scientific bodies, in a considerable number of which he has been honored with some official position. He has made numerous contributions to medical literature, the latest of which is a series of articles in the *Twentieth-Century Practice of Medicine*.

**John Collins Warren, M.D., LL.D.**—At the last commencement of Jefferson Medical College the honorary degree of LL.D. was conferred on Dr. John Collins Warren, Professor of Surgery in Harvard University.

**Albrecht Nagel**, Professor of Ophthalmology in the University of Tübingen, died July 22d, at the age of sixty-two years.

#### BOOKS AND PAMPHLETS RECEIVED.

Coal-mining and the Health of Colliers. By Simeon Snell, F.R.C.S. Ed. Excerpt from vol. xvi, Part I, of the Journal of the Sanitary Institute.

Clinical and Experimental Study of the Leukocytosis of Diphtheria. By John Lovett Morse, M.D. Reprinted from the Boston City Hospital Medical and Surgical Reports, 1895.

On Whooping-cough; its Great Fatality, and the Necessity for Isolation and Rest in its Treatment. By William W. Johnston, M.D. Reprinted from the Archives of Pediatrics, 1895.

Self-retaining Drainage Canula for Preventing Scar in Cervical Abscess. By F. M. Briggs, M.D. Reprinted from the Boston Medical and Surgical Journal, 1895.

Indian Medical Congress—Surgical Section. The Influence of Percivall Pott, Syme, Simpson, and Lister on Modern Surgery. The Address on Surgery, by Surgeon Lt.-Col. E. Lawrie, M.B., President of the Section. Hyderabad: Printed at H. H. The Nizam's Printers' Press, 1895.

Du Catarrhe Naso-pharyngien. By Dr. E. J. Moure. Extrait des Archives Cliniques de Bordeaux, 1895.

Wills Eye Hospital Reports, Vol. 1, No. 1, January, 1895. Published by the Editorial Committee of Wills Eye Hospital, Philadelphia. Philadelphia: Printed by the J. B. Lippincott Company.

Fifteenth Report of the State Board of Health of Wisconsin, 1893-94. Madison, Wisconsin: Democrat Printing Co., State Printer, 1895.

Xanthorun Diabeticorum, with Report of a Case. By J. F. Schamberg, A.B., M.D. Reprinted from the Journal of Cutaneous and Genito-urinary Diseases, 1895.

Malignant Endocarditis. By William Pepper, M.D., LL.D., and Alfred Stengel, M.D. Reprinted from the University Medical Magazine, 1895.

Angina Pectoris. By Herman D. Marcus, M.D. Reprinted from the Medical Bulletin.

An Interesting Case of Rapid Speech-development in an Adult following Operation for Tongue-tie. By G. Hudson Makuen, M.D. Read before the Philadelphia County Medical Society, 1895.

The Use of Scissors in Excision of the Tonsils, with Reference to a New Instrument. By Arthur Ames Bliss, A.M., M.D. Reprinted from the Therapeutic Gazette, 1895.

Wisconsin College of Physicians and Surgeons, Milwaukee, Wis. Circular of Information for 1895-96.

The Medical Treatment of Cancer of Larynx. By E. L. Vansant, M.D. Reprinted from the Journal of the American Medical Association, 1895.